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FOREWORD

This Magnetics Workshop was concerned with the measurement and reduction of spacecraft magnetic fields. It was sponsored by the National Aeronautics and Space Administration and was presented at the Jet Propulsion Laboratory in Pasadena, California from March 30 to April 1, 1965.

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INTRODUCTORY REMARKS

E. A. Gaugler
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NASA Headquarters decided to convene this Magnetics Workshop because we saw the need to bring together the various independent groups distributed throughout the country who are concerned with the magnetic properties of spacecraft. We sought to encourage them to discuss their particular experiences, to exchange ideas, and to provide the opportunity for other groups who are entering this field to be brought up-to-date on the state of the art.

In planning this agenda, by means of a working group consisting of representatives from NASA Headquarters, JPL, and NASA's Ames and Goddard Centers, it became perfectly obvious that we could not hope to cover the topic thoroughly in one 3-day workshop. Furthermore, the meetings in turn would stimulate new approaches to old problems. Therefore, we decided not to attempt to cover the entire field at this time, but to contemplate additional workshops to be held in the future at NASA Centers.

The lack of communications in this field has had an adverse effect on the development of space probes such as Mariner IV and Pioneer. Project managers have found it necessary to duplicate experiments and adopt a conservative attitude on magnetic specifications. Not only has this been costly in time and money, but it also has caused the growth rate of magnetic performance to be much slower than the actual state of the art would permit.

In the next few days, you will be hearing about numerous spacecraft in which mission constraints and state of the art have dictated widely different tactical approaches to the problem of constructing magnetically clean spacecraft.

To put matters in their proper perspective, I would like to mention briefly the situation of two flight programs with which I have been associated, namely, Mariner IV and Pioneer. Analogous situations undoubtedly occur within the other flight programs that will be discussed. In the case of Mariner IV, the primary objective was to conduct planetary experiments in the vicinity of Mars during a specific planetary opportunity. Interplanetary magnetic field measurements constituted a secondary objective and, because of scheduling pressures, we were unable to take the obvious

step of deperming* the entire spacecraft prior to launch. There was certainly little question that deperming was desirable from a magnetic point of view, and all the know-how and equipment were available to perform this operation. However, there existed a finite but unpredictable probability that deperming the spacecraft would jeopardize the entire mission, and we certainly could not afford to take that risk. However, deperming experiments will be conducted on the proof test model and this will enable us to take advantage of what we learn for the next Mars shot. This will certainly make an interesting topic for discussion at a future Magnetics Workshop. The IMP satellite and the current Pioneer series represent the avant-garde end of the magnetic spectrum. In these cases, the magnetic field investigation in interplanetary space is one of the prime objectives of the mission, and producing a clean spacecraft is of paramount importance. To date, IMP is magnetically the cleanest spacecraft ever flow – still, making it that way was a retrofit program. In other words (although steps were taken to keep it clean from the onset), at a certain point in time, its magnetic moment was measured and then active steps were taken to reduce its value to lie within acceptable limits. However, when Pioneer came along, we were able to build upon our IMP experience, and for the first time specific magnetic constraints were inserted into the program almost at the onset. Pioneer A is scheduled to fly later this year, and it will represent a new generation from the point of view of a magnetically clean spacecraft. Our experience to date has shown that significant advantages accrue in magnetic performance, time, and cost savings by introducing the required magnetic constraints early in the mission definition phase of the program.

In soliciting papers for this workshop, I encountered considerable reluctance on the part of some of the most knowledgeable people in this field to talk about their experiences. Some experts didn't believe they could afford the time to take what they consider to be provisional knowledge and refine it to the point where they would be willing to "cast it in concrete" for posterity. However, they did agree that if we would keep the tenor of the sessions on an informal basis, they would be willing to participate actively in the discussion periods. We plan to maintain an informal atmosphere, and that is why (in planning our agenda) we have been rather generous in our allowance for discussion periods.

*Demagnetizing.

I would now like to introduce Mr. Jack James, Acting Assistant Laboratory Director for Lunar and Planetary Projects, who has some remarks to make on behalf of the Jet Propulsion Laboratory, our host for this first Magnetics Workshop.

WELCOMING REMARKS

Jack N. James
Acting Assistant Laboratory Director
Lunar and Planetary Projects
Jet Propulsion Laboratory

We are pleased here at the Jet Propulsion Laboratory to act as your host under NASA's first Magnetics Workshop. I know that in my own experience, some of the most profitable experiences that I have had in project work have been the attendance at certain similar workshop meetings. I can remember one in particular, at NASA Headquarters, on spacecraft system testing. I think the OSSA*-sponsored project manager conferences have been equally beneficial to me. I have attended three of those; they were informal in nature. I feel the exchange that took place there was profitable to all of us, more so, frankly, than some of the more formalized technical society meetings.

I am impressed by some of the notable guests that we have here today. In fact, when I look about the audience, Dr. Gaugler, Dr. Sonett, and so on, I feel a little bit like the donkey that entered the Kentucky Derby. As he was waiting in the paddock for the race to begin, a thoroughbred looked over at him and said, "You really don't expect to win this race, do you?" And the donkey said, "No, I don't, but I do expect to benefit by the association."

I hope that I, and the rest of us here, will benefit by the association today and I will turn the meeting back over to the thoroughbreds in the field of magnetics.

*Office of Space Science and Applications

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RECOMMENDATIONS OF THE PARTICLES AND FIELDS SUBCOMMITTEE IN RELATION TO MAGNETIC SPACECRAFT

Charles P. Sonett
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I had rather mixed feelings about the workshop and what I am supposed to say; in fact, I still do. I don't know if I am one of the donkeys, I sort of feel that way.

I was at the meeting of the Particles and Fields Subcommittee at which there was a long discussion about the Voyager program — which in a sense is the pertinent one now — and what should be done about keeping Voyager magnetically clean. The Subcommittee came up with some recommendations. What I remember is a long discussion involving several points of view that I suspect, were not completely resolved.

Basically, there are those on the Subcommittee who feel that Voyager should be cleaned up so that one can carry out magnetically clean experiments. There is another group that feels that the problems are probably so difficult, because of Voyager's large size, that it might be better to have a different spacecraft system for deep interplanetary work, going out of the solar system. Of course, that problem is a programmatic one and revolves around how much money there is, etc.

I don't think that issue was settled, but the committee did come up with the following recommendations:

1. That specifications be placed on the Voyager design to enable interplanetary magnetic fields to be measured to 0.25 gamma on a Mars or Venus mission, and 0.1 gamma on a Jupiter mission. The spacecraft should be capable of measuring a 5-gamma field at Jupiter because present evidence indicates the field is something of the order of 5 gamma. At least, that is the best theory. There are some subsidiary recommendations in that regard that are applicable on a somewhat broader base than just magnetometers that apply to interplanetary experiments generally; namely, that the orientation of the spacecraft be known to better than 5 deg. I don't think that is a problem.
2. That a magnetometer weight of 10 lb be incorporated into the design.

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3. That mounting space be made available to view in three orthogonal directions, with considerable surface area available in one direction.
4. That a thermal control be incorporated adequate to protect temperature-sensitive detectors.

Now that I have done what I am supposed to do, namely, present the views of the Particles and Fields Subcommittee, I can say that I am not sure I agree with those views. If I were making a set of recommendations, I don't think I would restrict measurements to 0.25 gamma for a Mars mission. Rather, that present experiments can and should be carried out to something on the order of 0.1 gamma.

Basically, my somewhat negative feelings about the whole thing are that I don't think this kind of a problem is resolved on the basis of good wishes or hard work by the design engineers; it is a programmatic problem and is solved by a mission definition. As long as a mission definition says that interplanetary experiments are secondary objectives to planetary exploration, you sooner or later in the course of system design bump into a situation where you have to make a decision. The decision is going to be unfavorable to the secondary objectives. That's natural and right.

The only way to solve it is to institute primary objectives regarding interplanetary experiments. That's a matter I don't want to discuss the pros and cons of, but merely to say that (in my opinion) it's the only way to solve such a problem. I think the capabilities are available to produce a clean spacecraft or one that is clean at the boom outrigger. The political problems, however, are not that clear.

Now, actually, I don't have much more to say. Along with other experimenters, people who have carried out experiments on various spacecraft, I've had some very unfortunate experiences; I don't think relating these experiences is going to improve the atmosphere or help things out particularly. We do know that people like myself get shot at pretty vigorously when our experiments don't turn out right; so, naturally, we like to turn and pepper the rest of the world.

I might spend a few minutes telling you why I think magnetic fields are important. First, let me say one thing about design. I don't want to go past that one point because I'm not qualified to. I don't think one can produce a spacecraft that is clean — sufficiently clean so the magnetometer can be mounted on the main body; you have to go to a boom. Even when you go to a boom, you still require care in the design of the spacecraft, and the time-varying fields in the spacecraft are almost more critical than the static fields.

You can tolerate reasonable static fields if you can be absolutely sure they are static over many months.

Now, as far as why magnetic fields might be important and not really secondary objectives: Until 1908 there was only one magnetic field known in nature, and that was the field of the Earth. It was in that year that Hale discovered the field of the sun. It was after World War II that the first field was found outside the solar system and certain stars were shown to be magnetic; now there is quite a list of such stars. Then, shortly after the war, it was also discovered that starlight is polarized. One of the speakers later today, Professor Davis, is the originator of what is the most promising theory regarding that polarization: that it is due to a magnetic field in the galaxy — an interstellar field. Very recently, that is, within the last few years, evidence has mounted that there is a magnetic field on Jupiter. This evidence is due to the continuum radio emission from it, which can only be explained in a reasonable way by the assumption of a magnetic field on the planet.

So you see that there is a lot of evidence beginning to grow that fields don't just exist around the Earth; that they are important in the make-up of the universe. Now, if you look at the energy associated with magnetic fields and if you make a simple calculation, which has to be an order of magnitude calculation, you find that the energy density — that is, the energy in a unit volume in the interstellar gas — is more or less proportioned equally or distributed between the cosmic rays and the magnetic field and the starlight. They are both something of the order of 1 ev/cm^3. So if you want to talk about the structure of the galaxy, you have to talk about magnetic field just as much as you do about anything else; and this is not an ignorable quantity.

As far as biological effects are concerned and, of course, because planetary objectives appear to be the primary consideration for programs like Voyager, you can also exercise some imagination and consider the possibility that fields are important in the life process. Let me give you an example of that. The Earth's field is thought to reverse every now and then. Perhaps a million years ago, or several million years ago, the last reversal occurred. This evidence isn't perfect, but it is indicative of a reversal process. The field of the Sun is known to reverse. This may be something that is associated with dynamo mechanisms in general.

Well, if the field of the Earth did, in fact, reverse rather than just turn (that is, went through zero), there would have been a time at which there would not have been a magnetic filter to keep out cosmic rays; then there would have been only the

atmosphere. If you look at the numbers you find that for the present magnetic field — and it's decreasing now, by the way — the equatorial cutoff is about 40 billion v. If you remove that magnetic field, you have only left the atmosphere, and the cutoff there is something between 1 and 2 billion v. Therefore, the flux of galactic cosmic rays has gone up by a factor of 20 to 40. Then you wonder if this is genetically important over a long period of time.

If you look at solar flare, then the spectrum is so much steeper that the effect would be quite a bit more than two orders of magnitude. Now, you don't know what the solar flare incidence in terms of numbers per unit time or the spectral might have looked like at the time Earth's field might have gone through zero, but it is interesting to speculate that it might have been at least as important as now. There have been, in fact, considerations in terms of the evolutionary catastrophies that we know have occurred in animal species through the life of the Earth. For example, the end of the Cretaceous Period when the dinosaurs died off suddenly.

With the present estimate of the thickness of the Martian atmosphere, the atmosphere will stop only something like a 5-Mev particle. These numbers are not exact, but they are approximate if Mars does not have an appreciable magnetic field. Most of the galactic flux would penetrate to the surface of the planet and it would be under a constant rain of high-energy particles (both the galactic component and solar cosmic rays if those penetrate out that far). So there are reasons for thinking that, from a biological standpoint, magnetic fields have to be considered as much as do other factors, such as atmospherics.

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PROBLEMS OF MAGNETIC CONTAMINATION

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My situation is not very different from those of the preceding speakers. One thing about having a title such as "Problems of Magnetic Contamination," is that it gives you a great deal of latitude.

There are a number of things that you might expect me to discuss, and a good introduction ought to clarify for you what your expectations can be. You might ask why it is that I've been asked to describe or discuss the problems of magnetic contamination, so I'm going to briefly state my qualifications. I am one of the Mariner experimenters. We are a small group of people who, in the past, have attempted to measure rather accurately the small interplanetary fields in the presence of rather large and unsteady spacecraft fields.

My basic purpose is going to be to go into some of the problems associated with magnetic contamination from this particular point of view. You might anticipate that I would say something about what it means to an experimenter to try to analyze the data obtained from so-called "hot" spacecraft. So I will just make a few remarks in the introduction, so those of you who were expecting that will not be disappointed; then I will go on to the main body of the talk.

There are two things that I just want to comment on with regard to working with the data. You get a spacecraft like Mariner II, where admittedly, as Dr. Sonett said, it was not the primary objective to measure interplanetary fields. On the other hand, from a practical point of view, we spent 110 days getting to Venus. When we got there, there was no evidence of a magnetic field. So we ended up with no fields because of Venus that we could study; on the other hand, we had 110 days of interplanetary fields we could look at. So, from a practical point of view, you roll up your sleeves and do the best you can.

Now, you can well imagine that where you have fields that are, say, a factor of 30 larger than the fields you are trying to study, there are going to be some problems associated with the analysis of the data. I suspect we put in a substantially large collective effort that might have better been put into cleaning up the spacecraft in the first place.

The problem that I will refer to occasionally is one that Dr. Sonett mentioned. One of the distinguishing features of the Mariner II spacecraft field is not only that it

was fairly large, 30 gamma that halfway through the mission changed to 130 when one of the solar panels failed, but that it was quite unstable. Stability is an important aspect of this problem. I think that it is very important to talk about completely clean spacecraft, but it is well to keep in mind that experimenters could tolerate reasonably large fields; not excessively large, but fields on the order of perhaps 10 to 30 gamma, if they were really steady fields, if they were stable in the sense that they didn't change in time. This was certainly not true of the Mariner II magnetic field that changed something like 10 gamma over the first 5 wk of the flight.

You are all here, presumably, because you sincerely would like to see magnetically clean spacecraft, and I shouldn't have to convince you of that.

I obviously am not in the position to tell you how to go about the job, because we have been very unsuccessful ourselves in doing it. But the one useful thing that I can do is to tell you how not to clean up spacecraft. That's something I know about.

My experiences in attempting to clean up spacecraft have been rather negative ones. Now I would like to say right at the outset that I am going to be critical in a very general way. I am not attacking anybody's competence or integrity, nor am I restricting my critical comments to people here at the Laboratory. Let me point out that I have had quite a few experiences with other organizations in attempting to clean up spacecraft.

I have worked on the OGO series, on Ranger, and Surveyor, in addition to Mariner. So I don't want you to get the idea that the examples I am going to choose necessarily all reflect the JPL approach to things, in many cases they do not.

I'd also like to say that this is really a nonscientific talk. I think this is inevitable in view of the state of our knowledge. As Dr. Gaugler said, this information is very provisional, and there are many areas that we know very little about.

All of us have had just a limited experience. This is an interesting situation to be in because, if the talk is nonscientific, it does enable you to take a point of view — and a rather strong point of view. You can't take strong points of view when you are dealing with facts, but when you are dealing with opinions, that's a different matter.

So I'm going to take a strong point of view, and I hope the other people who follow me and have contrary opinions will also take a strong point of view; I think that's one of the values of a symposium of this kind. Many of the things that I'm going to say I'm sure people won't agree with, and they probably are partly the product of reviewing things, using the Biblical phrase, "through a glass darkly," because

I have only seen certain aspects of programs in action, and I've seen them through certain colored glasses.

What I would like to do is take a point of view that I think will complement Dr. Sonett's. I intend to discuss in some detail the general aspects of producing dirty spacecraft; I am interested not primarily in the technological aspects, because there will be a couple of days set aside for that, but in the general overall approach — in particular, what one might consider the program point of view.

Now, first off, the approach that I will take will be to describe some of the things that I think are required to do a good job, using illustrations of how a good job was not done in the past. I am sure that cleaning spacecraft up is a very difficult job, and it's going to get harder as the spacecraft get larger. Basically, you are up against the fact that there are so many ways to fail to make the spacecraft clean and apparently so few ways to succeed. It is not, probably, unlike any other engineering or scientific task in that respect. But I think there are certain basic ingredients that go into successfully cleaning up a spacecraft. I would like then to take a fairly large view and discuss, from the ground up, what I think is required.

Now one crucial aspect of the experiment is, of course, close cooperation between the experimenter, the Project Office (which is responsible for the spacecraft at the center), and NASA Headquarters. This is obvious.

A lot of the things that I am going to say are obvious; but, nevertheless, and it will sound slightly irrational to you, let me point out that on the basis of some of my experiences, some strange things do occur. The trouble with organizations to clean magnetic spacecraft is that, like most organizations, they are made up of a lot of people. And I'm reminded of a comment of Mark Twain's: "One thing about dealing with human beings, you know exactly where you stand; things couldn't be much worse." So, I'm going to say a few things that may not sound quite logical to you; but, nevertheless, they do happen and I think it is because human beings are somehow involved in this whole process.

The responsibility of the three different members of the team is quite clear. The experimenter is not responsible for cleaning up the spacecraft. This is something that people are sometimes confused about. We are frequently attacked on this basis. The responsibility of the experimenter is to establish certain requirements to carry out his scientific objectives. Then he should follow the plans to see that these objectives are carried out, help translate these general goals into specific items and then assist in seeing that the goals are met and that the fields are within

acceptable limits. The experimenter obviously is in no position to clean up the spacecraft; he doesn't have the resources. It is all he can do to get his experiments ready in time to meet the schedules; the only person who can do this job is the spacecraft manager. The spacecraft manager obviously cannot relinquish his authority over an area of design that is so critical and one that involves so many tradeoffs between competing requirements.

Because these tradeoffs are involved, and because many of them do affect the scientific objectives in a very direct way, NASA Headquarters has to play this very important role. They negotiate between the manager and the experimenters and arrive at some kind of a priority list. Explicitly or implicitly, they must eventually decide which things have to be emphasized. Now that is the way the system ought to work, and by and large it does tend to work that way. I am sure that many of you are already aware of some of these things, and many of the things that I have to say are already well-known to a lot of you. I hope there will be a few things that none of you have heard before, something that you will get out of the talk as I go on.

For this sort of cooperation to work, one really needs to eliminate confusion about the roles that the different groups are to play. Because, if there's any unwillingness to play a certain role, then the whole cause of magnetic cleanliness is jeopardized.

I'd like to describe a few of the hazards that are involved for each of the people, as I see it. First of all, the experimenter has to generate realistic requirements; he has to avoid insisting on more than he really needs. The point of view that most experimenters would take today sound, on the surface, unrealistic to most engineers. It's very easy today to build magnetometers that can measure fields as small as a tenth or a quarter of a gamma. Naturally, it is perfectly rational from the experimenter's point of view to say, "I don't want any contamination. When I can measure fields that small, why shouldn't I be permitted to do it? What's the problem?" This is perfectly reasonable. If he really wants to measure the interplanetary field accurately, he can maybe tolerate an instability of 1/4 gamma in the field. As I said, if you had a pretty stable field, even if it were 10 or 30 gamma, and you knew it was going to stay steady over periods of 6 mo to some fraction of a gamma, you'd be happy about it, provided you knew exactly what it was before launch.

Stability is an area about which very little is known. There is plenty of evidence that on certain spacecraft, at least, these fields change rather radically.

The alternative, then, is to insist that the spacecraft fields be small, because if you have a 100-gamma field, and it's unstable by only 5%, that represents a 5-gamma change. It's as large as the average value of the interplanetary field.

Where the experimenter is forced to compromise is in connection with the planetary and lunar missions. If your purpose is really to measure fields just near the planet and not worry about interplanetary measurements, then the requirements are much less stringent. I think the experimenters will all agree to this. The difference being between the so-called absolute measurements and relative measurements. If you want to measure the interplanetary field, you have to make absolute measurements; you want to know the magnitude and direction of the field very precisely. You want to know what it is compared to zero.

Now, if you are going to look for just a planetary field, you look for a change from interplanetary conditions to things that are characteristic of the planet. Those are relative changes you are looking for; just changes in direction and magnitude. And all you really require is that the spacecraft fields not change over the period of time it takes you to get by the planet, or there not be fields associated with the planetary part of the mission that will mask your measurements and make you think that the television system or the attitude control system looks like a planetary field to you.

I'd like to now take up some of the hazards associated with spacecraft managers. One thing that I really feel strongly about is that the spacecraft manager must take the experimenters' requirements seriously, and honestly attempt to meet them. No spacecraft manager, out of hand, says "We can't do that, and we're not going to make the effort," but these compromises continually creep into the program. This is not entirely his fault; there is certain prejudice, on the part of his staff, that experimenters (who they may not have had much contact with) really sort of directly descended from the academicians who used to count the number of fairies that danced on the head of a pin.

I'm sure they get lots of laughs. Everybody says "Ha ha! They get 0.1 gamma field. How unbelievable how unrealistic — too bad these people are so ivory-towerish." Our reply, of course, is that we notice that all sorts of things, which technologically seem extremely complicated to us, get done on the spacecraft. We're not as impractical as we sometimes appear and our requirements should not be compromised just on that basis alone.

One of the other thing :hat creates problems is this matter of the priority list once it's drawn up. One .akes ou a list of priorities of something like ten items, and then very often project people (and I'm not talking about the manager, but right down the line) will use this in a way that was not intended. His staff, for example, may start with a dozen objectives, and promptly and effectively proceed to disregard the 2nd through 12th as inconsistent with the primary objective; these things occasionally do happen.

Now priority lists ought to be designed to resolve conflicts and not just to eliminate them. NASA Headquarters is implicated in all this, too. After all, they are responsible for determining the mission objectives, and it should be clear somehow to outsiders that just the point of view that Dr. Gaugler expressed here this morning, sometimes compromises are necessary. If you want to get to the planet and do a planetary experiment, then maybe you make the interplanetary measurements secondary. That has certain implications, and the experimenters, the project manager, and NASA Headquarters, then, are all in the same boat. They all have to be prepared to defend what they have committed themselves to. In the final analysis, they share the blame for what happened.

Well, so much for the working team that starts the thing off. Presumably, these three groups can arrive at a set of magnetic specifications that are mutually acceptable. Now, with very few exceptions, magnetic specifications have been treated more in the nature of design goals. What's happened is the engineer has been told he ought to try to make his package clean, this happens on lots of spacecraft, and it's true of AC fields as well as DC fields. There may not be any real firm specifications. Then, when the experiments are delivered (or the subsystems are delivered), they are promptly measured and then an attempt to fix the top 10% of the worst of the lot is made – somewhat after the fact. Hopefully, this sort of thing can be avoided if, as Dr. Gaugler says, there's a clearly defined set of specifications at the outset, and NASA Headquarters and the project manager are prepared to take a very firm stand on these specifications.

Magnetic specifications, like other specifications, are not very popular and it's been my experience that there's probably going to be several people who will get these specifications and promptly say, "That is a lot of nonsense! They aren't going to make me meet these specifications. I have got all these other constraints on me – reliability – I'm trying to build something with minimum weight and minimum power." This actually happens, people build systems with complete disregard to

magnetic specifications. I know of one attitude control system built that consisted of a lot of huge rotating magnets when it was completed.

There is no doubt that the burden cannot entirely rest with the design engineer. He is out in the field and somebody sends him a specification. He says, "What do I know about magnetic fields? I know about communications systems." and he sort of flounders; he would sincerely like some help, and the project office has to have the resources to provide him with that help. There have to be people who can go to him and help him understand his problem and suggest standard techniques for doing something about it. There ought to be some kind of a body of documentation that one can point to and say, "Well, these documents prescribe the standard techniques that one uses." I think that's certainly a hopeful sign, that there is a conference of this kind, because this is an area where we really need a lot done.

Magnetic cleanliness is really pretty much of an art; as some would have it, a black art at this stage. I suspect from the spacecraft manager's standpoint, and I know from the experimenter's standpoint, this causes a lot of gray hairs. No spacecraft manager can be really sure that he's got the resources to clean up the spacecraft and to meet the requirements of the experimenter; this tends to undermine the experimenters' ability to have their views prevail.

So the technical aspects, apart from some of the human engineering or programmatic aspects, are certainly essential. What I'm saying is based on the fact that I assume it's technologically possible to clean up the spacecraft. The work that you people in the audience are going to do over the next year or two will greatly increase our knowledge about such things as degaussing*, shielding, and stability.

While I'm on that point, there were a couple of technical comments that I wanted to go on record as to my position on some of these things.

I think degaussing is a very promising technique and, in many cases, there is probably no real good alternative to trying to degauss the packages. As you will undoubtedly hear, this is an area where a great deal more needs to be known. It has been done successfully on some occasions and unsuccessfully on others. This is really the area where we need good qualified processes that we can fall back on.

The discussion of magnetic shielding of hot parts — magnets, components, and what not — is something where there is considerable difference of opinion. This difference of opinion is because so much of our knowledge is provisional. Here at the Laboratory we have had quite good success in shielding materials, fabricating

*Degaussing — means demagnetizing in this paper

packages that have the proper shape to guide field lines, and annealing these properly; we are pretty well convinced that shielding will work successfully.

Now, it's fairly obvious that one cannot build a completely nonmagnetic spacecraft, unless people can start making wooden transformers of some kind. It is surprising to me the large number of things that turn up in communication systems. Consider, for example, circulators, traveling-wave tubes, transistors very often are magnetic, welded circuit modules are magnetic. So there are bound to be magnetic materials on the spacecraft.

You can try to degauss these. Some of them require magnetic fields as part of their operation. Communication circulators, for instance, must have magnetic fields; traveling-wave tubes must have magnetic fields. Those fields have to be shielded and, by and large, it is an advantage to the design engineer to do just that. He does not benefit from having stray fields scattered all over the universe. In many cases, some specific cases that we've been involved in, it's been possible to greatly increase the efficiency of the device and more than compensate for the increased weight because of the shielding by appropriately confining the fields to the active volume — the area where the design engineer would like it to be, where it's going to be useful to him.

There are going to be magnetic parts on spacecraft and something has to be done with them. If they are going to work as magnets, then they have to be shielded. I think the second implication in this, as Dr. Sonett mentioned, is that you've got to get the magnetometer sensor away from these parts. There is no substitute for distance. You have got to take advantage of the inverse cube law and you've got to have some kind of a long boom or perhaps you have to put the magnetometer sensor and whole experiment on some kind of a small subsatellite — some small substructure that can be ejected from the spacecraft once it has been placed into the trajectory.

One other thing concerns the emphasis that should be placed on magnetic fields associated with currents. I am sometimes concerned about the tremendous effort that goes into magnetic materials as such. And yet, things can slip through that produce fairly large fields associated with currents, just because people didn't think to keep the effective areas of any loops that are present small, to use bifilar wiring, coaxial wiring, or twisted wiring wherever it was necessary — wherever there were wires carrying a substantial amount of current. Certainly this has been a problem, and it seems to me something that is fairly easily corrected in most cases, if proper wiring techniques are used. Yet it seems to get second priority

when there are magnets of one kind or another that have to be degaussed and shielded against.

Finally, I'd like to make just a few more comments; I want to turn to the measurement of the subsystem fields and the spacecraft field itself. As you know, if you have specifications (or even if you don't have them), if there are effective design goals, when the subsystems are delivered the laboratory has to have some kind of facility in which to map these things. They have to see whether people have met their specifications or if they have to be fixed, if completely incompatible with the scientific magnetic experiments.

The other thing is to have some measurements of the entire spacecraft field. You have to make some measurements, either to demonstrate that you have achieved your goal of very low magnetic fields (essentially zero magnetic fields), or that you have not achieved it; you have to measure these fields.

Now I've been involved most of my adult life in attempting to measure magnetic fields because of spacecraft. The thing that I've been impressed with, really, is the difficulty of making good measurements before launch that you can rely on after launch. This is what we have tried on several occasions; it is very difficult to do. Usually, one of the problems is that one cannot get a whole spacecraft in any one location and measure the magnetic fields due to the DC materials, measure the fields due to the currents, and have the solar panels on and deployed; in other words, the spacecraft in a flight configuration. In the past, there have always been a tremendous number of compromises that force you to measure each of these things piecemeal. So you end up making four or five measurements, all accurate to some fraction of a gamma if you are lucky; then in the end you try to stack all of these things up, and it seems quite clear to me that it is hopeless to think you will come up with something less than a gamma.

I don't think the techniques you use are really important. The best one is to take the whole spacecraft and place it in a magnetic facility where one can reduce the field to zero or to some known and controlled value. Here at the Laboratory, I think quite successfully, we have used the technique translating and rotating it about two axes in the Earth's field.

Now, the other thing associated with making these measurements is the problem of stability. What happens to the spacecraft field after the time you make your final mapping and the time that it gets into space? Who can guarantee the magnetic environment for that interval of time while it's being transported to and from the

gantry, when it's being placed on the rocket and when it is shaken in the Earth's field at one hundred g or something? We have a little bit of information about the gantry, incidentally. There are some people here who have measured the gantry, in case there are a few of you who are interested. The fields don't seem to be frighteningly large, about the magnitude of the Earth's field. We had some bets going on how many orders of magnitude these fields might be, but they are not really too bad.

But these are the things that the experimenter worries about. What this means, basically, is that once you launch the experiment (and, of course, the spacecraft goes along with it) you have to have some means for determining what the spacecraft fields are. The scientific community probably isn't going to be satisfied by anything less, and may be reluctant to accept the results anyway. But you are in a much better position if you can say, "These fields were actually determined in space."

Now, the way to do this (there only seem to be a couple of ways) the one that has been used most frequently in the past is just an implicit part of the spacecraft performance; namely, that most spacecraft are attitude-stabilized so they are rolling around one axis. Now, if the spacecraft rolls long enough, of course, you take advantage of the changing orientation of the fields you happen to be in, whether it's geo-magnetic or interplanetary with respect to the spacecraft field. One would get some kind of a sinusoidal variation where the amplitude of the sinusoid represents the interplanetary magnetic field, and the DC level represents the spacecraft field. The spacecraft fields rotating with the spacecraft contribute a DC term.

There has been a problem in this regard and there will be a problem, I presume, on planetary missions, because the communications requirements almost seem to force one to stabilize the spacecraft. Some of the larger spacecraft, even the Earth satellites, are stabilized. So how does one go about determining these fields if the spacecraft is not allowed to roll? This has been a very serious problem. On Mariner II, for example, there was a very large change in the spacecraft field between the time it was mapped prior to launch and the time it got into space; the only reason I think we were able to get many of the results out of the data that we did was because there was a period of several days right after launch before the spacecraft was attitude-stabilized. We were able to determine two components with quite good accuracy. Even if you are permitted that much, though, there is the

problem of the other axis, because you cannot get information about the spacecraft fields along the third axis.

Now in the past we have suggested, just sort of offhand, that one ought to have some kind of a programmed maneuver to roll around that other axis. This always produces an interesting reaction.

Well, I think this is a serious problem that really hasn't been thought about as much as it should be; you have got to demonstrate that you have zero field. This is an implicit part of doing any good experiment; it is not enough to get up and say, "Well, we have done it," because there are always critics in the audience.

I would like to see a very frank and open discussion of the requirements and techniques to be used in achieving magnetically clean spacecraft.

COMPETING SPACECRAFT DESIGN CHARACTERISTICS

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My role in the Mariner Mars project and some of the earlier Ranger projects has been spacecraft project engineer and, as such, I've been one of the primary targets for some of Dr. Smith's comments; although I did think over the past 5 or 6 years we were pretty good friends. I remember a couple of weeks ago, back at NASA Headquarters, Dr. Smith was making a presentation and he referred to himself as one of those persons who earns his living by measuring magnetic fields; whereas I belong to that class of person who earns his living by creating fields for him to measure.

Both Dr. Smith and Dr. Sonett made reference in their talks a couple of times to a list of competing characteristics or a list of priorities, which is the topic of my talk here. I thought by way of introduction or background, I might describe what form this list of competing characteristics or priorities takes.

At the outset of any project, of the complexity with which we are dealing here, it's necessary for the project office to establish a set of ground rules from which the design can depart. These ground rules generally consist of a statement of what the mission objectives are, what the mission restraints are, and the design criteria that are to be applied.

Mission objectives would include a statement to the effect that we would like to go to the planet and take pictures, measure the magnetic field of the planet, or perform some other kind of planetary experiment. A secondary objective might be to make interplanetary measurements. I am emphasizing this point that Dr. Sonett made earlier, that in the past the interplanetary experiments have been treated as secondary objectives from a mission point of view. A mission restraint would be a statement regarding the launch vehicle that the spacecraft would be required to use, what the allowable launch period would be, and things of that nature.

The design criteria — and this is where we are more concerned — would include statements on the design approach, the general design approach, to be taken with regard to the spacecraft. It would be a general statement to the effect that it would be a fully attitude-stabilized spacecraft, perhaps, or a spin-stabilized spacecraft. Whether or not we use RTGs or solar panels for power, etc.

There would usually be a statement about reliability criteria, and this I will have more to say about later, because I think this is the area where magnetic requirements most strongly conflict.

There is a statement on the schedule criteria, this is not really a competing characteristic in the sense that if you are faced with a planetary program or project. There can be no compromise as far as the schedule goes; you have got a launch period that may be 2-1/2 or 3 yr in the future. That launch period will be 2 to 4 wk wide. If you are not down there on the launch pad with the spacecraft at that time, you don't make it; you don't get another chance for another 18 mo or 2 yr, perhaps longer for some of the more distant planets.

There will be a statement about weight. Spacecraft weight cannot exceed some established value that has got to be established early in the design, and that is established on the basis of the vehicle performance, which is another fixed parameter, and the minimum launch period that the project office is willing to accept.

And then there is a list of competing characteristics, and these competing characteristics might go something like this:

1. For a Mariner IV-type mission, our primary objective would be to arrive at the planet.
2. Next in priority would be the successful operation of all planet instruments.
3. Another competing characteristic might be continuous telemetry throughout the transit part of the flight.
4. A fourth one might be successful operation of the interplanetary instruments.

Then you might find compatibility with future projects or later missions on there, and the list goes on.

Well, getting back to the reliability criteria, I'd like to expand on that a little bit because, as I said before, most of my comments will relate to our attempts to satisfy the magnetic interference and the relation of those attempts with the reliability criteria that we established for ourselves.

One criterion for reliability that you might practically find yourself having to live with as a project engineer is to use, to the greatest extent possible, hardware that has previously been developed for some other project. There are not only economics involved with that, but there is the advantage that hardware has been previously developed and you have had a chance to work the bugs out of it. Probably

there has already been some life testing on the project; you know the weaknesses; you know what the problems are with that hardware; and you can attack the new evolution of this hardware, so to speak, with the idea in mind of fixing what you know to be wrong, rather than facing a whole new set of problems. So, usually, we find one of the most fundamental reliability criteria is to use, wherever possible, previously developed equipment.

Another reliability criterion is that in the design of the spacecraft and its associated subsystems there has to be provision for complete and adequate ground testing. If you are to have any assurance that the equipment is going to work in flight, it has to be testable on the ground, and you have to demonstrate that the design you propose to launch 1 or 2 yr hence has a high probability of success.

Another reliability criterion might be the statements on the use of redundancy. It might give tradeoff values through application of redundancy versus weight or power. Another criteria might be to maintain or devise functional independency among subsystems, so that the failure of one system or subsystem won't induce unnecessary failures in other parts of the spacecraft.

Now, where does magnetic interference or quality fit into all this? I haven't mentioned it. These are the ground rules that we start out with. These are ground rules that are usually established by the project office; and as project engineer and design engineers, we have got to then establish the design restraints that we are going to apply at the hardware level on each subsystem, and these restraints have got to be compatible with those foregoing mission objectives and design criteria.

The conflict of magnetic interference is not so much with the established competing characteristics as it is with the criteria for established reliability. That is especially true, I think, with the use of previously developed hardware and provisions for adequate testing.

I would like to discuss about four aspects of system design or spacecraft design. They are all related, these four are related to each other, and they are all, in turn, as a group related to spacecraft reliability and magnetic cleanliness. These four are the general design approach that we might take in any particular area, the selection of components that might be used in the design, the selection of materials, and the selection of processes.

Now, concerning reliability, the selection of design approach, once again, bears on those two reliability criteria. We may be constrained to select a particular design approach because it has been used in the past; there has been some previous

development along these lines. Or we may be forced into selecting a particular design approach because it provides the capability to be easily tested on the ground. The design approach then is, to a large extent, affected by our criteria for reliability.

Selection of components. What does it mean to select a reliable component? The reliable components are especially in the area of electronic components, components that have been in production for a long while, components for which there is a great body of statistical data available. We have our lists of preferred electronic components that have been developed over a period of the last 5 or 10 yr, which brings all this experience (in terms of field experience, etc.) to bear on the selection of components that can be expected to give a reasonable probability of success in space application.

Selection of materials. Here again, the same comments apply to reliability. We have got to select materials for use whose properties are understood; materials that we know how to handle. It isn't so much that a particular material may not be handable, but the techniques for handling it might not be sufficiently developed at the time, and within the time scale of the projects you don't feel you can afford the luxury to go into the research and development necessary to make those materials usable.

Selection of processes. Finally, what techniques or processes are we going to permit to be used in the spacecraft design? I think this is probably one of the most subtle areas in my experience where we have really run into the most problems, in the control and use of processes and assembly of spacecraft, and in the treatment of materials.

So those four items, then, are constrained considerably by our desire to build a successful system.

How do they affect the magnetic considerations? I think later on I will give some specific examples in each of these areas that are largely taken from our Mariner IV experience. The design approach to magnetic cleanliness might be where do we locate the magnetometer? Should it go on the end of a mast? Should it go on the end of a solar panel? Should it go on the end of a boom?

The components, I think transistors are an item that Dr. Smith mentioned earlier; we find that many of the transistors that are reliable components are encased in metal cans that have unfavorable magnetic properties. Dr. Smith has already mentioned the use of traveling-wave tubes, circulator switches, and RF components.

Materials – Stainless steels that were generally thought to be nonmagnetic in the past, we found (on using them), actually have magnetic properties (some of them) and special stainless steels have got to be used.

Processes — Use of potting compounds, for instance, or honing techniques (liquid honing techniques) will give rise to magnetic contamination in many cases.

I think what I would like to do now is to touch on each one of these areas and cite a few examples where we have run into problems of trying to satisfy simultaneously our reliability goals and our requirements for magnetic cleanliness.

As far as the location of the magnetometer goes, I don't think there has been a preliminary design or design study conducted here at JPL, (or anywhere else) that has not, at one time or another, considered the use of booms for the location of the magnetometer; by the same token at least, none of the designs that have been pursued to the hardware stage at JPL have actually incorporated a boom. We have been questioned about this many times, and I can only state that the following considerations have been applied and the decision as a result of these considerations has always been unfavorable, as far as the use of a boom goes. We find it necessary to consider the failure modes. If you are going to locate a magnetometer on a boom, usually because of the shroud constraints, the boom has to be an extendable or erectible boom — something that will be erected after the spacecraft is injected.

It is necessary to consider the failure modes. The cg of the spacecraft is different with the boom in the stowed position, versus the boom in the extended position. Now, failure modes have to do with the failure mode as far as the magnetometer itself is concerned. If the boom fails to extend when you have got a spacecraft with a 30 gamma field (which is about what the field of the Mariner IV spacecraft is) and you are expecting fields, down to 1 gamma or so, if the boom doesn't extend, you compromise the mission as far as the magnetometer goes. But you might also compromise the spacecraft as far as performing a midcourse maneuver, because it is necessary to very tightly locate and control the location of the cg.

There are autopilot interactions that come into play during maneuver that have to be carefully guarded. Center-of-gravity shift is one of them, dynamic disturbances is another. A long boom or a magnetometer out on the end of a long boom can produce dynamic disturbances. When the midcourse maneuver motor is first fired, there will be an acceleration that will cause a deflection of the boom, and then under steady state conditions as the boom comes back, there will be a tendency for some component in the autopilot to saturate, such as the gyros. I am sure that, given enough time, a problem like this could be worked around.

Another consideration is the stability of the autopilot. These long, flexible structures have resonances that, unfortunately, generally tend to lie in the area of autopilot interaction. For any given resonance, of course, it is probably possible to

design an autopilot that can accommodate it, but there are also solar panels and other flexible items that are not so easy to get around. They generally don't have the same frequency response or resonances, and the problem of providing an autopilot that can handle all of these is an involved one; not one that couldn't probably be solved in the future.

Other considerations, as far as the design approach goes, would be loop currents. Now, we have paid strict attention to the effects of loop currents on the Mariner IV project. We have done this, of course, by keeping the return currents close to the supply lines and using twisted pairs wherever required. We got into considerable problems with twisting. We found that twisting the wires, of course, is the best way. There have been some proposals for a novel weaving of wires that produces more effective cancellation, but we have run tests that indicate that a tight twist is better than any other approach we have come across. However, the wire insulation — it is necessary to get the twist tighter than the normal twist you find in commercially twisted wire. Twisted wire off-the-shelf comes one to one and half turns per inch. We found to get effective cancellation you need approximately three to four turns per inch, but this requires special techniques. It is necessary to use planetary twisting, which is not usually provided (in ordinary twisted pairs at least), and the insulation that we find required to satisfy our specfications' on outgassing is not always compatible with the tight twist. We have found cases where the insulation would crack under the strain. Here again, you get into a problem where you have to make a compromise. You can't twist it at four turns per inch, which is the effective and maybe the best twist, because of the insulation cracking; then we have to back down to two or three twists per inch.

Another problem, as far as the design approach goes, might be to avoid loop currents; not only in the wiring itself, but in the structure. On the Mariner Mars there is a low gain antenna mast, a metallic tubular structure that sticks forward on the spacecraft and is the structure on which the magnetometer is located. This is supported by a couple of struts. If there was metallic contact on all three points of support, there would be a possibility of a loop current being induced. To avoid this, it is necessary to insulate at one of the three joints.

We have been concerned over the loop effects that could possibly be created by the thermal insulation of the spacecraft cabling. It is our practice, usually, to wrap the cables with aluminized Mylar or aluminized Tedlar. Here is an opportunity for a loop current condition, and special precautions were taken to avoid this. Well, so much for the design approaches.

The problem of selection of components and materials can be conveniently broken down into two or three sections, maybe — mechanical, electronic, and RF components. They all have slightly different characteristics and slightly different problems. Perhaps the easiest problem to solve in the future will be in the area of mechanical components and materials.

As I mentioned, we have to go to special steels, stainless steels, for fasteners on the Mariner program. These were procured by special order. We used special steels or titanium for bolts to circumvent the magnetic problem, but we got into operational problems with the titanium bolts at least, because the titanium tended to gall; and this caused us even more problems and we finally had to go back to steel.

On the connectors — An ordinary connector that has an aluminum shell has a steel wave washer for providing compression in the mated condition. We found it necessary to order connectors with special nonmagnetic wave washers.

On the wire we used — For instance, ordinary wire has tin plating for solderability, but the extrusion processes and the temperatures required to handle the Teflon coating are too hot for tin, and so the ordinary wire is supplied with a nickel coating. You have to watch out for that; and we found that we could order special wire with silver coating rather than the nickel, but it has to be specifically called out that way, or you get nickel wire and you wonder why.

Bearings that are used to support the solar panels were specially procured in the Mariner IV design in nonmagnetic materials. Bearings, incidentally, are going to be a real problem. Wherever you find it necessary to have a bearing, it turns out that any of the nonmagnetic steels cannot be hardened sufficiently if the unit loads that need to be taken out by the bearings are heavy. I believe it's presently true that we know of no nonmagnetic real bearing materials. Fortunately, this was not a problem with Mariner IV because bearing applications that we did have (or the one significant one) were the scan platform, and this had very light unit loads and we were able to use an asbestos sleeve bearing; but in general, I don't think the problem can be circumvented that easily.

I think in the future bearings will be an area where we will have considerable problem.

Bimetal elements that we used for thermal actuators and temperature control — During the time of the hard design and hardware procurement phase of the Mariner IV there were no nonmagnetic bimetal materials available. I understand there is one

available now, however, and we can look for some relief in that area. Temperature transducers tend to be magnetic, but can be procured in nonmagnetic versions.

One of the most insidious problems that we ran into was in the area of conformal coating and potting compounds. We ran into situations where potting compounds used to pot the back shell of connectors and the conformal coating material that was applied over circuit boards were actually magnetic; this magnetic contamination was traced to metal particles that got into the compounds, generally from storage containers or from the tumbling process that was used to mix the compounds during manufacture.

However, the problem of this general class of materials and components is gradually alleviating as manufacturers are becoming aware that there is a requirement and need for nonmagnetic components and materials. In many cases, a lot of this material can be procured off-the-shelf. Down at the bottom of the page there will be a footnote or statement that such and such part is also available in the nonmagnetic equivalent.

In the area of electronic parts, however, I don't think the solution is going to be quite so easy. As long as there is a requirement for relays, actuators, and gyros, we are going to have magnetic contamination problems. I don't know whether shielding is the answer; there is going to be a talk on that later in the workshop seminar, which will discuss some of our experiences with shielding in detail.

I might mention that in the area of the central computer and sequencer, which is one of the subsystems on the Mariner IV program, this was one that was almost a direct carry-over from Mariner Venus; latching relays were used extensively in the interface area. We attempted to shield these magnetic relays by putting little five-sided Mumetal cans over them. This shorted the internal magnetic circuitry of the relay. The shield provided a nice flux short, all right, and cut down the external field, but the relay wouldn't work. We made attempts to procure nonmagnetic transistors. If any of you have had experience with those, you know what the problem is in maintaining the hermetic seal.

We had very bad luck from a reliability standpoint in trying to procure nonmagnetic transistors. The same is true with nonmagnetic tantalum capacitors. There is a manufacturer that does offer a capacitor made of nonmagnetic material, but it has a very poor failure rate compared with other commercially-available capacitors; and hence, does not compete very well, as far as the selection of components goes.

On the other hand, we had a few incidents where we were able to get, and with some success, components that are ordinarily very magnetic, in a clean version. We use a motor-driven switch to turn spacecraft power on and off. This switch comes from the manufacturer in a steel can. We found that we were able to replace the ordinary can by one made of Mumetal, and that worked very nicely.

Microswitches are another source of magnetic contamination. Microswitches, as provided by the manufacturer, have a cold-rolled steel base; these can be procured by special order with nonmagnetic bases.

Another area where we ran into trouble was the field of RF components. When our spacecrafts were working at L-band this was not so much a problem, because at those frequencies cavity amplifiers, which have no magnetic circuitry to speak of, are perfectly adequate. However, with the conversion to S-band, which has been taking place over the last couple of years, we find that we have to go to amplifying and RF switching techniques that depend extensively on magnetic materials and magnetic fields.

To meet this requirement, and at the same time provide a magnetically-clean component, we launched into a very large development effort, about 4 years ago, to develop a 25-watt klystron amplifier working at S-band. To meet the functional requirements, the klystron had something like a 5000-gauss field in the gap, and a 1-gamma field externally at 3 ft. So, magnetically speaking, it was a smashing success, but as far as a klystron, it didn't work at all.

There has been a more recent development of crossed-field devices, such as an amplitron tube, and (by special precautions) it is possible to get the field down to about 60 gamma at 3 ft, this is about the best we have been able to do; but this is a tube that operates at about 10 w.

We look toward the future — toward Voyager. We are going to want to operate at a higher RF power levels and this is going to mean stronger magnetic fields, and we can expect larger external fields, even with the best precautions.

Traveling-wave tubes (TWTs), as mentioned, have periodic permanent magnetic focusing and the one that is on Mariner IV now was a crash development as far as incorporating it on the spacecraft. It was necessary to back up our cavity amplifier design. Because of the short time schedule, we didn't take any particular precautions with the magnetic field on the TWT, but even so the external field was only about 30 gamma. So it may be possible to do some real effective shielding with TWTs. Klystrons and the crossed-field devices, I don't think, are too promising.

The cavity amplifiers at S-band above power levels of 5 or 10 w are completely out of the question because of the low efficiency.

Circulator switches — another item that was mentioned before as an example where an effective magnetic circuit design can work. I think I take a little exception to one or two of the remarks that were made that shielding has got to be provided and shielding can be provided effectively.

I think, and maybe you would agree with me, that the answer is not so much shielding but efficient magnetic circuit design. If you can confine the magnetic lines to the area where they have to interact with the RF fields, you are way ahead of the game. Then maybe use the shielding as an extra little crutch. But to try to start off by depending on shielding, I think is suicidal.

This brings us to some of the problems that we have experienced in our processing, which I would like to mention. You find that you have to be concerned with the cutting tools that are used in the machining process of any parts. You might take a piece of nonmagnetic material and machine it and use a magnetic tool in the lathe, and when you get the part in house, you've got a magnetic part because it's got some chips or filings embedded into it.

Liquid honing, which is a technique that we used here, caused us some problem at first until very special and extensive precautions were taken to keep magnetic contaminants out of the honing liquid. Gold-plating is a process that can cause problems with magnetic contamination, because the standard technique for gold-plating will be to nickel-plate on the base metal before gold-plating.

Now, there is a process that can circumvent this. It is an electroless nickel-coating technique, which is a galvanizing or dipping process. We haven't any experience with it here in flight applications, but it looks promising.

The use of welded cordwood modules as an assembly or packaging technique — this is bad, not only because weldable materials tend to be magnetic, such as nickel, but because of the way that the nickel is stacked up in the module. The components are all laid up like logs and then the nickel ribbon is welded across the ends. All the dipole or magnetic moments of these individual welding strips tend to add and contribute to magnetic contamination. Yet welded cordwood construction, we find, is the most reliable from a packaging, processing, and component-treatment point of view.

Lastly, there are the magnetic tools that are used in the assembly of the spacecraft — screw drivers and pliers. You take a titanium screw and drive it home

with a metallic screw driver, you leave a little chip from the screw driver embedded in the screw head, and if this is in the vicinity of the magnetometer, you find you have a 2- or 3-gamma interference.

What I have tried to do here is to summarize some of the problem areas we ran into trying to satisfy the magnetic constraints. Although I didn't specifically point it out in each area, I think you can see where any conflict that existed between the magnetic constraint and our reliability consideration, the reliability consideration usually won out.

In summary, I might state that our basic job, after all, is reliability. Even to the subsatellite that was mentioned, the same problems are going to exist on the subsatellite that exist in the spacecraft. Granted, it will be on a smaller scale, but it is still going to have to have an attitude control system of some kind, a radio system of some kind, and a power system of some kind to service the magnetometer. Because it is smaller, the magnetometer is going to be closer physically to these supporting subsystems, and I am not sure that that really is a clean solution. I think what we have to do is to attack the problem at the level of the components and the materials and the processes; and I think we are making progress there.

The way to ensure both high reliability and good magnetic quality, I think, involves recognizing the source of magnetic problems early in the game and avoiding them. Now that is where we have been the hardest hit, because this is the first time that we have been through it (I'm speaking of the past few years) — we don't have much experience. It's the first time, really, we have been through the design exercise where magnetic considerations are really important.

You don't know in advance, for instance, that your potting compound is going to be magnetic unless you take special precautions to avoid contaminants. We have now built up a background of experience and we know what to look for and the problem areas to avoid. If we take advantage of the collective body of knowledge that probably exists here at JPL and the other centers, and apply this to the future design, such as Voyager; if we make our selection of materials and processes early in the game, recognizing the problem areas, then I think we can go a long way toward providing the type of cleanliness experimenters are looking for.

My one last closing comment is that one of the biggest decisions that has to be made is when you carry a piece of equipment through preliminary design, prototype design, you have already started your parts procurement and screening, then one of the last things you do by way of environmental testing on the prototype is take

it to the magnetic facility, and you suddenly find you've got a horrible magnetic problem.

What do you do at that point? You go back and you find that all the transistors that you used are horribly magnetic, or you've got to change your fabrication process or technique. Well, to institute a change like that at that point in the design is almost suicidal.

If I might, by way of an apology to my colleague, offer that as probably the most contributive reason for the dirty spacecraft that we have today; and once again, the way to avoid that is to make an intelligent and knowledgeable selection of the processes and the materials early in the game.

OPEN DISCUSSION

VOICE: There was a mention that the titanium used in place of stainless steel galled. What was the mating material in that instance?

MR. CASANI: It was the steel inserts, I believe, the A-286 inserts.

VOICE: Have you tried titanium on any other materials to see whether you got galling in that instance?

MR. CASANI: You're at the heart of the problem. Dr. Smith looks to the project office to provide the resources to solve this problem of magnetic cleanliness; but at the same time the project office can't undertake a big development effort in the middle of a project. We have to use components that we understand and that can be procured easily, and whose properties are well understood. In this instance, we were committed to a particular type of fastener to be used in the spacecraft. It was a nonmagnetic stainless steel that was practically in production, so to speak, and we went around looking for a nonmagnetic bolt to use. We tried the titanium for its non-magnetic properties as well as the weight reduction approach. But the titanium was not compatible with the insert, and so we had to back off.

Now I think that the point you raised is a good one, that there ought to be some advanced development effort going on in the area of packaging and processing materials to provide us with the ready-made tools, so to speak, to carry out a project development.

DR. SONNET: Well, I am not sure if it is a question that I have or a comment, or both, but let me start with a question.

Is it true that the design philosophy at JPL generally is opposed to the use of a boom, and is it likely that this will continue?

MR. CASANI: No, I don't think that our philosophy is opposed to the use of the boom. We have used booms in the past. There was a boom used on the Rangers III, IV, and V spacecraft, as you remember, for one of the science instruments. But, in the past, where consideration for use of a boom, as far as the magnetometer has been concerned, – all things considered, the outcome has been negative. Now I think I would agree with you that to get the type of fields that you are talking about, booms are going to be necessary. But we must recognize that the use of booms interacts with other aspects of the mission objectives. And, unless those objectives are set forth in their proper order, I don't know that the use of booms is going to be very prevalent.

DR. SONETT: It is very difficult, of course, in a conference, to get into the design aspects of the boom problems. As I see it, there are essentially the problems you mentioned. There is the problem of cg offset and of boom damping. I guess those two were the critical issues.

MR. CASANI: That and stability; the resonance stability of the boom.

DR. SONETT: That is what I mean by damping. Now, I have never seen a real study of this. But, for Mariner II, the problem was brought up in that case, and it was an opinion kind of decision, rather than based on studies. I have not seen, to this date, and I suspect from what you said, the following is true: that there really isn't a system tradeoff that has been done – a detailed system tradeoff that tells what it's going to cost to clean up the spacecraft so you can stay close in, versus going out on the long boom. Now, to my mind, and I am treading on dangerous ground here admittedly, I don't see that the problems of a long boom are insoluble. The cg offset problem is one of putting a torque on the spacecraft while you are in maneuvers.

Now, because on Mariner IV the midcourse maneuvers were completed by about 10 days out (if I'm right, that's 1/25 of the total mission time), I'm sure the experimenters would be willing to give that up and leave the boom undeployed until midcourse maneuvers were over, then deploy the boom.

Then you have the problem of properly designing the damping. It may be a serious problem in coupling with the other problems, but you really want to consider the tradeoff of that versus all the tremendous number of other things that you have talked about that would have to be done.

MR. CASANI: I think I would agree with you, Dr. Sonett, but I would also like to point out, taking a little more long-sighted view toward future projects, such as Voyager, you must recognize that maneuvering will be a mission requirement in the vicinity of the planet. So, once again, you have got the same problem. What do you do? Retract the boom. The failure modes that are involved at the terminal phase of the mission then become important.

I think that I agree with you that perhaps a detailed tradeoff has not been done.

DR. SONETT: Don't you think that 4 or 5 years downstream is a little bit late for us to be talking about the need for a tradeoff study; that this is something that should have been done much earlier? Because JPL is committed to a particular kind of attitude control system, an inertially stabilized type of system for its program. As far as the use of a boom — there are, of course, other considerations that you can take into account too because you can remove the boom in the vicinity of the planet — get rid of it. Because planetary measurements aren't the same kind as the interplanetary.

Secondly, in the problem of attitude control in the vicinity of a planet, which is different from the midcourse corrections, you can, for example, reference the OGO's case that is under continual midcourse direction, because it has to orbit the Earth and take on a particular direction all the time.

MR. CASANI: Is that the one that the boom failed on?

DR. SONETT: Well, that is a catastrophe caused by a different problem.

MR. CASANI: Well, those are the problems we have got to consider. I think that I agree with you, but, of course, I am not in the position to answer for all the JPL policy by any means. But your comments are well taken, and I think tradeoff studies of this type will be undertaken for the Voyager.

DR. SONETT: You see, the reason I ask these is not to be needling, but soon — sometime in the next year or so — there will be RFPs coming out to the scientists, and they will be asked to propose experiments. A very pertinent question is whether one wants to propose an interplanetary magnetometer experiment — and I am not just speaking for myself, but I am speaking for the community at large, for Voyager. Is there any point in it?

You can take the point of view that you would propose and you do the best you can. Or, you can decide that there are many programs more meaningful and more promising in this kind of an experiment. And, in my mind, because I believe that

you have to go to a boom, it would be very helpful if, in that RFP, we knew that kind of thing; and I suspect we might not, because we are still a bit upstream, is that right?

MR. CASANI: Yes, I suspect that you are correct. I also suspect that unless the measurement of interplanetary fields becomes more of a primary mission objective than a secondary or tertiary objective, the use of booms will still not be competitively favorable with fixed location on the spacecraft. This is an informal gathering and that is why I felt I could go out on the limb that way.

MRS. EBERHARD: You mentioned a problem of galling. What were the circumstances on that?

MR. CASANI: Well, the particular application was the spacecraft subsystems are packaged in subassemblies and are bolted to the main structure, and the female fasteners are made of stainless steel on the main body of the spacecraft. Titanium bolts were used to bolt the subassemblies onto the structures by running the bolt into the female fasteners, and it was the galling in the titanium that occurred in this application.

VOICE: That was a stainless-titanium product rather than a stainless-aluminum.

MR. CASANI: That is right.

VOICE: Are JPL preferred parts lists, process specifications, and the source information for things like your magnetic relays or your solenoid valves, etc. available?

MR. CASANI: I don't know if I could answer that question completely. There is a considerable effort going on right now in our process and materials section to develop this sort of thing. I think there is going to be a speaker later on in the program; is there not?

CHAIRMAN GAUGLER: I think Mr. Iufer from Ames Research Center is going to talk about preferred parts.

VOICE: You mentioned the problem of nonmagnetic bearings. I'd like to point out that Mr. Buehler is giving a talk on Thursday and he will talk about a material that has been used for this.

MR. CASANI: This is Nitinol?

CHAIRMAN GAUGLER: And I think also for nonmagnetic tools.

VOICE: I assume that one of the competitions you have is sort of economics as to what you can investigate and what you get for it. We have had somewhat similar problems with mine sweepers, and we found it useful to have the yardstick, "with so many dollars you get so much reduction," in the field, and then you can apply relatively uniform policy on the efforts that you are putting into it. Have you tried something like this?

MR. CASANI: No, we haven't tried anything like that in this area. We have a similar technique in the area of weight reduction. We have kind of a rule of thumb — so many dollars per pound. I think part of the problem is that it is difficult to assess how much improvement each step you take is going to provide before you are committed to the step. In other words, very often you might go through a whole shielding routine on the magnetic relays and spend all the money, in effect, to develop the shield and everything, and find out the relays don't work or the shielding isn't effective.

What I am trying to say is I don't believe we have found here at JPL — maybe some of my colleagues will take exception to this later on — a way of predicting what result any given step you might take toward reducing the magnetic fields quantitatively will have.

CHAIRMAN GAUGLER: I might add — on the Pioneer program there is a magnetic incentive, so dollars are traded off with magnetic gammas.

VOICE: I'd like to make a couple of observations from talks that have been made this morning. There seems to be lack of motivation for all this effort to make the spacecraft magnetically-clean. Apparently, we would like to measure the interplanetary magnetic fields as well as the rest of the planet; yet there seems to be a great deal of objection to equipment that might be required to do it in interplanetary travel or the difficulties in using it.

I was wondering if somebody might comment a little more on the motivation of all of this effort to make it magnetically clean.

MR. CASANI: I don't know whether that is a fair or not. If I understand it properly, I would say the motivation is the same as for any other design or restraint we establish. We basically, I think, are all trying to do what the job requires.

The magnetic restraint is one of a list of several dozen restraints that are put on the subsystem and designs. There is restraint put on radiation background; we don't want people using radioactive materials in their subsystems, but it has been done in the past.

There are restraints on techniques to be applied, etc.

VOICE: I thought putting on magnetic measuring equipment is the reason why we would like to reduce the magnetic field of the spacecraft. That is the motivation.

DR. SMITH: Are you interested in scientific objectives? In other words, the motivation to do the experiments?

MR. CASANI: That is different question than I was trying to answer. Maybe you want to take that?

DR. SMITH: No, but I will make a comment or two. I think Dr. Sonett made some comments about why measurement of magnetic fields in interplanetary space is important; and if all goes well, Professor Davis, who is also one of the Mariner magnetometer experimenters at the California Institute of Technology, is going to make a few comments at the end of the next talk. I think he intends to say some further things about this. So perhaps it will become clearer to you.

Admittedly, more could be done along this line, and my point of view is that we're all here to clean up the spacecraft; we experimenters should do more to convince people of the importance of making good magnetic measurements. Perhaps Professor Davis can help you out there.

VOICE: I think we all understand that is the motivation, but the objection seemed very strong, and I think some effort will have to be done to maybe find a different way other than the boom, or perhaps putting it out on the end of a leash. Some imaginative thinking in this area will have to be done, cutting it loose, maybe.

DR. SMITH: The experimenters have discussed many of those suggestions with the design engineers in different aspects of this program, and I think Mr. Casani's point is the same as ours: that it's all fine and very well to sit around and throw out these ideas, but there ought to be some really serious thought given to the study of the different possibilities.

The pattern that has been established by IMP and currently being established by Pioneer is quite clear. This is the comment that Dr. Sonett made, that the only

way that people have successfully reached these very low magnetic fields at the sensor is to take a very strong position at the outset in the importance of the magnetic measurements and do everything they could to clean up the spacecraft and then to resort to long booms.

VOICE: Is there a possibility of making a fixed magnetic field, a stable one? In other words, in creating a magnetic field in the spacecraft rather than try to reduce it; in other words, masking all the other fields?

DR. SMITH: I wouldn't want to try it.

VOICE: I just wanted to mention a second motivation, outside of the experimenters for magnetically-clean spacecraft. We are now concerned with spacecraft that have very severe attitude requirements — Earth orbiters — and because of the moment of the spacecraft, you get interactions with the Earth's magnetic field that can produce a torque. And so, even if the experimenters weren't here, there is a very practical reason for continuing on with this magnetically clean spacecraft concept.

MR. HUBBARD: Hubbard from Naval Ordnance Laboratory. Is there in existence in NASA some committee for materials perusal as there is in the military, as an aid to design people for improving the magnetic background of these systems?

CHAIRMAN GAUGLER: I don't know, it would probably be under the auspices of the Office of Advanced Research and Technology. I don't know the name of any such organization.

MR. HUBBARD: Do you think this might be worth considering?

CHAIRMAN GAUGLER: That's a good idea.

MR. HUBBARD: The military has come up with this problem a long time ago.

CHAIRMAN GAUGLER: One of the things that we hope to come out of this workshop is the direction for future R & D. In other words, so that each project doesn't start out from scratch and do all the things everybody else has done. We definitely have in mind, and we hope to come up with, NASA specifications. For example, on what is the moment of a spacecraft; how do you define it; are you talking about moment or are you talking about gamma at so many feet; after deperm or after perm, or what?

So I think this will probably generate out of this sort of a meeting. That's a good idea.

MR. WEINBERGER: Weinberger from General Electric. Do you have any information on batteries, especially nickel-cadmium?

MR. CASANI: I don't think we do have any information on that. I might, refer you to our battery people. But here's an example I think, where I might take a chance to illustrate a point — There is a considerable effort for the development of sterilizable components, components that can be heat-sterilized. And, the battery that you mentioned is a good example. There is a great deal of effort being expended here at JPL now to develop a battery that can be heat-sterilized, as well as other components; but there is, to my knowledge (except in the area of the RF power amplifiers), no special effort being devoted to the development of magnetically clean components. And, I think that this is certainly a recommendation that this group might come to here in the next 3 days, that such provisions be made for that type of advanced development work.

Now, in the area of the RF components, I think that I mentioned that these S-band devices all depend on the use of magnetic fields, and we do have a development going here at JPL to develop an electrostatically focussed RF power amplifier operating at S-band with a power level of around 100 w or so. This is the sort of advanced development work that ought to be more prevalent, and it is something we might all give some serious thought to. Pressure has to be brought to bear on the subsystem discipline areas, and there is that type of pressure built up now for sterilizable equipment, but not for nonmagnetic.

CHAIRMAN GAUGLER: That's a very good idea; and I also think that pressure has to come from Headquarters to define the magnetic moment of the spacecraft in the mission definition phase of the program. I think if that's put into the actual definition, then I think this will prevent a lot of difficulties from occurring later on.

MR. PARSONS: Parsons from Goddard Space Flight Center. In answer to the question back there about battery data. There is data available at Goddard. I have some data with me today. I am sure that data in other areas have been tested. Basically, the answer is, if it's a nickel-clad battery, don't fly it.

MR. WOOLLEY: Dick Woolley, Ball Brothers. Because you do have nickel-cadmium batteries in your spacecraft, you spoke of contamination because of liquid honing, for instance; do you find that contamination is significant in comparison to your nickel-cadmium? Can you detect that it is there?

MR. CASANI: We don't use nickel-cadmium batteries; we use silver-cadmium.

MR. WOOLLEY: Still, on this contamination because of liquid honing, is it something you can detect from as much as, say, 6 in. away by any techniques that we have?

MR. CASANI: Very definitely. That's the point. That is what is so insidious about this whole problem; you get stabbed where you least expect it. Who would ever think of worrying about liquid honing or potting compounds or magnetic contamination of the components' coating material, or if a man uses a screw driver that has been lying near a loudspeaker, that this picked up a slight perm and he goes and uses that to tighten up a bolt. These are the very areas, I think, that do tend to produce problems that we have had, and that's where it is so tough to attack (because you can't sit back and think of all these things before the fact); it isn't until after the equipment is built, and the processes are established, that you track down what is causing the contamination. It is too risky to fix it, so I think that as our body of knowledge and experience builds up in this area, we will be able to avoid these problems by wise selection of processes and materials that will avoid the problem.

MR. PEIZER: Peizer, Naval Ordnance Laboratory. I think the need for a specification — we found it very useful with the sweepers — that the Chief of Naval Operations set up a definite field limit for the ship before it was constructed and, as a result of this, we were able to apply pressure to the people that handle the money, and say, "If you don't do this, you won't meet the limit." The plans and what was proposed could each be examined to see how they fell in respect to the limit that was set up. If we had no limit, I think the thing would have gone to pot.

CHAIRMAN GAUGLER: I think what's happened in the past in some of the NASA programs, there has been a limit set, but it hasn't been adequately policed.

DR. GOREE: Goree from Stanford Research Institute. Have you considered the magnetic fields created by currents caused by thermal EMFs about the spacecraft and between various components?

MR. CASANI: I don't think that we have looked at that very carefully. That is a very good point. I don't know how to answer it except that, of course, you need junctions of dissimilar metals with thermal gradients across them before you've got the problem; whether or not that could be a problem with that Mariner IV spacecraft that is up there right now, I don't know, because I don't think that we have looked at that.

DR. GOREE: There are pretty severe temperature gradients, aren't there?

MR. CASANI: There are severe temperature gradients, but you have to satisfy the other criteria too.

DR. GOREE: Well, you can get some EMFs along the single material between the impurity dislocations.

MR. CHRISTY: I just wanted to say that we haven't done this on Mariner IV, but this has been done on part of the solar panel failure autospy on Mariner II, and the thermal EMFs there should be detectible, although they should not be very great. I think this amounted to fractions of milliamperes or something like this.

MR. BREWSTER: Brewster from General Motors. I'd like to ask the experimenters, primarily, what the magnitude of these instabilities in the magnetic fields are and their duration. You've talked about booms being the only solution. Or a solution and you've mentioned some of the instabilities but I'd like to get a better idea of how big they are and how rapidly they occur.

DR. SMITH: Our only experience was with the Mariner II spacecraft. Over a period of about the first 5 wk, we saw a continuous – almost continuous – change in what we interpreted as the spacecraft field. And, over the first 2 wk, the change was on the order of maybe 2 gamma and then it steepened and during the next 3-wk period, something like 5 gamma, so that we had about a 10-gamma change after about a 5-wk period. Then, there was some kind of a very rapid change again in the field that lasted something like 1 wk, or a little bit less; and then we had this very large solar panel failure. Now, that is the evidence from the experiment in space. I might say that on Mariner IV we seem to have a much more stable spacecraft. Over the 100 days of the flight, as far as we know, we have seen no evidence of any shifts in the fields. I think that is certainly a measure of the improved management on the Mariner IV spacecraft.

MR. OSTHELLER: Gary Ostheller, JPL Systems. There was a question previously on motivation. I am wondering if it is motivation or if it all hinges on money. To get a magnetically clean spacecraft, you almost seem to have to up the priority to almost a primary priority. If you have a primary priority of just a deep-space probe, then you would also probably do away with the requirement of doing a maneuver, which would then make a boom more feasible. But to be able to do this project requires a primary definition from NASA, and also requires more money; more money for research into a magnetically clean spacecraft.

So the question is, does NASA, or can the scientific community make NASA feel that they should spend the money to pursue this?

CHAIRMAN GAUGLER: Well, I think that is a very good question. I think that it is a lot more money to try to clean up a dirty spacecraft; in other words, if you are going to get to a certain level, it takes a lot less money to get to that level by starting out and know you are going to get that way than to start out at some very high value and then try to clean it up later on.

But, there's no doubt about it, NASA has been somewhat lax in defining the magnetic constraints in the beginning of the program and convincing the people — the project managers — that they really mean it. Because I know one of the programs I've been associated with — it was very specifically called out that you shall have a certain background level, but unless you just got in there and watched it like a Navy inspector, you just didn't get what you started out to get.

VOICE: I was wondering if the sad experiences are documented in any one place, or whether there is a committee at JPL that goes through analyses of this sort of documentation; the same sort that you would go through on part failure analysis documentation.

MR. CASANI: I am afraid that there really isn't, except that we have a history of all the magnetic properties that were measured on every piece of flight hardware that was flown, and an analysis of what contributed to the magnetic contamination in each area. Now that much has been done, but beyond that, I'm afraid that there is not this type of history that you are speaking of.

MR. GRUMET: Grumet of Republic Aviation. Dr. Smith mentioned that his magnetometers had readings that jumped appreciably after launch. I was wondering if any one of the investigative reviews restricted the effects that caused this. We checked on completely degaussed Mumetals in a shielded environment — where if we stressed the Mumetal at all it set up sizeable magnetic fields.

DR. SMITH: No one ever really determined what this apparent change from prelaunch to postlaunch was due to. There were a large number of suggestions made. The little evidence that was available in the data suggested that the changes were probably associated with current loops, and what this evidence consisted of, primarily, the slow changes that I mentioned in connection with the other question. The changes that took place before launch to the afterlaunch condition tended to have a preferred direction in the spacecraft coordinate system, and this direction tended

to be the same as the field change that was seen when the solar panel shorted out. There was a large, unmistakable field effect associated with that. It was a jump in the field something like 100 gammas when we lost one of the solar panels, and then the solar panel started to operate again for a period of 1 week, and then it ceased functioning. So, we had about three of these changes. It did appear from that data that it was most likely some kind of a current loop; but beyond that I don't think anybody has been able to diagnose in any greater detail what it was.

REMARKS ON THE NEED FOR INTERPLANETARY FIELD MEASUREMENTS

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I want to make a few brief remarks about the magnetic problem as it appears to a theorist who has been associated with these attempts to measure the interplanetary magnetic fields. I apologize for not having been here from the beginning, and perhaps I will repeat what has been said. I won't regret this, because I think that these points need all the emphasis they can get.

First, very briefly, I want to mention a few of the specific reasons why we need to know the magnetic field to within at least 1 gamma (and preferably within 1/4 gamma) whenever one is making measurements of the magnetic field outside of the magnetic field of a planet. It isn't just the desire of the experimenters to have a fine-sounding experiment, it is essential if you are going to do several important things.

One is to determine the structure of the interplanetary field. It clearly has a spiral structure. To determine it properly, one must know the spacecraft field within 1 gamma, preferably to 1/4 gamma. Also there seems to be a north-south component to the interplanetary field. These factors are very important in determining the propagation of solar cosmic rays to the Earth.

Still it is more important, actually, in understanding the interaction of the solar magnetic field and of the corona; that is, in any theoretical interpretation of the interplanetary field, one needs to know the spacecraft field with this accuracy. There are several important topics in plasma physics that can be studied — certainly best and perhaps the only way they can be studied is in the interplanetary region but they are of interest to people in wide areas of science.

Shocks are one example of this, the collisionless shocks can be studied in interplanetary space. To do this properly, a theorist needs to know the field with the accuracy that I have mentioned. The fields probably show zeros, or as they are usually called, nulls. At these ranges, there are very interesting and puzzling possibilities that the field lines should rearrange their connections — a very important problem in modern plasma physics. And again, if you are looking for a place where the field is zero, you have to know what the spacecraft is contributing. These are samples that I won't elaborate on because I don't think this is the interest here. But

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I hope I give you some concrete feeling that there are definite and specific reasons for this requirement.

Next, a short remark that I feel is very important, based on all the experiences that I have had in this area; that is to get a field of 1/4 to 1 gamma with any confidence, that you know it with this accuracy, needs both a very serious attack on the magnetic contaminations and it also needs a serious study of booms and subsatellites.

If one is going to make magnetic measurements in interplanetary space, I feel that a boom is as important as a solar panel. You can't do it without a solar panel, and you can't really do it without a boom — and I don't think it's any more difficult to design. But you can't just toss it off and decide what you do about it in 10 min.

I feel that the experimenters who have worried about this for 5 years now, should be included in some of the preliminary discussions of booms, rather than being first brought into the discussions after the features of the spacecraft are all set. I think that the booms must be considered before the attitude-stabilization design is frozen, for example.

I think if the effort that was devoted to a study of booms beforehand were comparable to that which goes into the reduction of the magnetic data from something like Mariner IV, that this would not be an unreasonable distribution of emphasis, and it would be a very profitable one.

Well, after this harangue, I go on to further reasons why the small fields are so important, of a somewhat different kind than I mentioned earlier. Even though magnetometer experiments can live with reasonably high field strengths, and I think we can do quite useful things — you see, we are in this awful dilemma. If we say you can't do anything unless you have this low field, then they throw you off the spacecraft.

If you do the best you can, then they say, "Why do we have to build you any better spacecraft than the last ones?" The reasons for this are that it is very difficult for us to reduce the data on spacecraft that have substantial fields. Every time you see an interesting little wiggle in the field on Mariner IV, you have to go back and ask yourself, "Is this real or is this something the spacecraft is doing?"

If you knew the fields were low, you would be much better off.

Another thing that should be of interest to everybody who is concerened with this, is that the results obtained on these spacecraft (which do not have small fields)

are not accepted by a large part of the scientific community. In presenting results obtained with Mariner II, we are repeatedly attacked with the claim that any result that depends on a knowledge of the spacecraft field is completely unfounded. I have heard it seriously urged by those having substantial influence in the selection of payloads that mangetometer observations should just not be allowed on certain magnetically dirty spacecraft. Anybody who is designing a spacecraft, I think, must take this fact of life of the experimenters into consideration.

The last remark along these lines, while it was stated as a joke was only stated half as a joke; Geophysical Research should be asked to print, in red, articles based on spacecraft having any substantial magnetic field, "magnetically-dirty spacecraft." The implication is that most of the scientific community then would not read these articles.

Well, this is all I want to say to emphasize the very great importance of these discussions that you are having here.

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ESTABLISHMENT OF SPACECRAFT MAGNETIC CRITERIA AND DESIGN RESTRAINTS

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Any restraint of any sort that goes on the spacecraft is a general restraint and goes in a formal document, that we have called, in past projects, "Design Restraints." The "Design Restraints" is a document that is generated and released by the project engineer. I am not the project engineer; that is why I have to qualify my competence here.

This document, although the project engineer may (and probably does), have inputs from a number of people from all the interested areas, neverthless, is controlled by the project engineer and the project management. This implies that the restraints that go in are compatible with the list of competing characteristics that Mr. Casani talked about. This is an important consideration.

The restraints that go in this document – there really are two – there is a spacecraft field restraint that is, in some ways, relatable to the experiment's requirements. But, this is not an implementable requirement. The spacecraft is not designed in detail at the system level. It is functionally designed at the system level, the detail design is done at the subsystem level. Therefore, this spacecraft field restraint, to be implemented, must be translated into a subsystem or assembly level field restraint.

This, in essence, is what I wanted to talk about; how in the past we have made this translation from a spacecraft field restraint to a subsystem or assembly field restraint.

When this translation is made there are three things that must be observed in making the translation, if your magnetics interference effort is to be successful.

The first thing is that the requirement must be formal. However one puts on subsystem restrains, it must be a formal requirement that has the stature of a project policy. When you put on a magnetics requirement this often implies that there will be some tradeoffs to be made: weight, cost, perhaps schedule, and perhaps even reliability.

The engineer must be able to infer that when a request comes to him for magnetic control that the person who has made that request has authority and is aware of these tradeoffs that could occur, and they have been considered in the design restraint.

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The second thing, which is very important if this effort is to be successful, is that the restraints must be meaningful to the eingineer. The first step in making them meaningful, of course, is that you don't give him a spacecraft field restraint and let him decide what part of it he can contribute. You must translate them into a restraint on his particular piece of hardware. You can extend this and say that when you have given him a restraint, he must understand it and it must be meaningful to him. You also have to give him a procedure, and perhaps even a facility for measuring magnetic fields. The engineer cannot and will not pursue a problem that he can't get his hands on.

Another thing, which we made the mistake on at least one past program, was that we gave a subsystem requirement and then said this is the field that you may contribute to the spacecraft field. By the way, when I talk about spacecraft field, this is the field that is measured at the magnetometer location. To us, this is pretty meaningful. We knew how far a peice of hardware was from the magnetometer experiment. To the engineer, it causes a lot of confusion.

First off, he might or might not know how far he was from the magnetometer; more important, we had given him a firm requirement — very firm — with a big variable in it. So he had to decide for himself what the magnitude of the variable was.

The third thing that goes in and must be reflected, is that the restraint must be reasonable. There has to be a fighting chance of meeting the restraint. If the restraint imposes a work load or a design effort that is far out of proportion to the position of magnetic interference on the competing characteristics, you will get no cooperation. The management will find itself with a firm requirement that they can't enforce, because it is impossible to meet.

As Mr. Casani pointed out, one of the standard practices to achieve reliability is that you try to innovate or do a product improvement on a past piece of hardware to get your next piece. You try to improve its flaws, and the same goes for magnetics. If you take a piece of hardware that, perhaps, was designed without a magnetics restraint and try to impose too severe a magnetic restraint, you do one of two things: (1) the innovation is too severe, and you have lost the reliability, or (2) you have lost the cooperation of the engineer, because you have asked him to do something unreasonable. I might add that a restraint that is too stringent is sort of a function of time. A restraint that is too stringent on a 2-yr design effort might be reasonable on a 4- or 6-yr design effort.

So let me recap once more these three policies that you must recognize when you set your design restraints.

1. That they have formal stature; that they are a formal requirement that you mean.
2. That they be understandable, and that the engineer can measure his progress in meeting these requirements.
3. That they are reasonable; that he stands a chance of meeting them, and he doesn't look upon them as a completely unreasonable burden and one to use subterfuge to get around.

Here at JPL we have flown magnetometers before. On the Mariner flight to Venus in 1962, there were no formal magnetics requirements on this effort. And, in fact, there could hardly have been, because this project used much hardware in existence from other flights; it was a very short-duration design period. So it is, perhaps, not a good expample.

On Mariner B, which was cancelled in 1964, we reflected two of these policies; we lacked one of them. The requirements were formal on Mariner B, and they were stated so that they were meaningful, and an engineer could measure his compliance with them. But the magnitude of the field allowed was probably very unreasonable and never would have been met. It was far, far less than the field of the hardware that would be modified to do a Mariner B. It was not compatible with the current or even the predicted state of the art in making field requirements.

On the current Mariner mission, the flight that is on its way to Mars now, our initial magnetic restraints again had two, but not the third one. They were formal restraints, and they were fairly reasonable and probably could have been met. But they weren't stated in such a manner that an engineer could understand them.

We talked about stabilities, and we talked about fields of a frequency of less than 10^{-3} cps; I have to submit that an engineer would not know whether his field was greater or lesser than 10^{-3} cps; and it carried two different requirements, one above that frequency, and one below. These restraints were modified then on the Mariner Mars mission, turned into what I feel is a very nice set of design restraints. They still were formal; they were stated so that they were meaningful. There was a capability available that an engineer could do work in — a facility, that is — and they were fairly reasonable.

So I'd like to talk about what these Mariner magnetic design restraints were. They were not set as we would hope them to be set in the future. They were not set by a subdivision of the spacecraft magnetic field. Following the launch to Venus in 1962, all of the spares from that project were magnetically mapped as a sort of

research effort to try and find out what sorts of things caused hardware to be magnetic. The results of that mapping showed that about two-thirds of the assemblies measured had a field of 5 gamma or slightly less at a distance of 3 ft and the other one-third had fields considerably larger.

Because the Mariner Mars flight, to a large extent, would use the same packaging techniques and improved versions of the Mariner Venus hardware, we didn't feel it was reasonable to try to make a revolution on a project that would have a design period of only a few weeks or a few months before the design would be firm. So we chose a value of 5 gamma measured at 3 ft for the perm fields. The 3 ft is sort of arbitrary. Three ft is as close as any of these subsystem would be to the magnetometer. Most of the things would be farther away than 3 ft; which means, of course, they would contribute less than 5 gamma. Those things, about two items, that were closer than 3 ft, were tagged as being special effort units that had to have a more stringent magnetic restraint put on them.

Mariner carried a limit on the current field of 0. 5 gamma at 3 ft. As Mr. Casani pointed out, the state of the art in reducing current loop fields we felt to be more advanced than in reducing perm fields; and therefore, it carried a much more stringent current loop field requirement.

I'd better mention that this requirement was for fields of DC to 5 cps. Because of the nature of the experiment, fields of greater frequency than 5 cps were not significant and there was no attempt made to control fields of greater frequency than that.

Mariner carried another restraint that we felt would be very important; this was a restraint on stability. We specified that the field of an assembly could not change by more than 0. 5 gamma as a result of going through a vibration test, or as a result of switching modes, in any of the modes from which science data was taken.

We went further, made a requirement and arrangement that these units have their perm field mapped before and after shake tests on the first available unit, and that the current field be measured on the first available unit.

On future projects, I hope there is a better way to do this. Perhaps some of it will come out of this workshop. It is desirable to tie the subsystem requirements in more directly to the spacecraft requirements. As I pointed out, we did not do that on Mariner. We used a rather pragmatic approach instead. Voyager is an example of a future project that makes a good point; to compare that with the current Mariner. Voyager will have about 1 1/2 yr from start of system definition to start

of hard design. The current Mariner and the Mariner Venus shots were both in the hard design concurrent with the start of the project.

OPEN DISCUSSION

MR. SCHNEYER: Schneyer of Lockheed. How many of the subsystems that you specified actually meet that 5 gamma requirement?

MR. CHRISTY: I think it ran something like 60 or 70% overall – something like that. That is only an estimate; we have got stacks of data on it.

MR. BASTOW: There are probably about 10 or 15 individual items that exceeded 5 gamma. Why? I suppose they were getting pushed to get it launched at the proper time. But some of these particular items were the welded nickel ribbon on modules. There were three of the eight bays on the Mariner that had some of these on them. So, each of these three bays exceeded the field. The ion chamber was one of the items that was within a few inches of the magnetometer and it consequently exceeded the revised requirement at that close a distance; so, it contributed a significant field at the magnetometer.

The solar panels initially did; however, we were able to deperm them – and several other items. The tape recorder had shafts and bearings that were creating an excessive field. I can't just offhand right now remember each of the items. The temperature control louvres, I believe there are four of them, around the main bus all contributed a significant field. It hasn't been mentioned yet, but the field at the magnetometer because of the spacecraft was in the order of 30 gamma; so these contributions – the largest one was possibly 18 gamma at 3 ft, and the majority of them were considerably less than that.

MR. BROOK: Bob Brook from Republic Aviation. You mentioned the necessity for formalizing these restraints. Did you write a specification and, if so, is it available?

MR. MACLAY: These restraints were formal. They went in a formal document, which is known as the design restraints. This document covers all general explicit restraints on the spacecraft, on all elements of the spacecraft. There was a supporting document that was written. It was a design specification. It was sort of a home remedy book; gave information on designing of nonmagnetic hardware, such as layout of circuit boards, and listed dirty metals that we'd hoped

people would avoid. I don't believe it ever reached the stage where we could list nonmagnetic electronic components in it. You asked if it was available. I don't know the answer to that. I would think that to obtain that, you would have to go through the Mariner Project office, to obtain either of those.

MR. MOSKOWITZ: Moskowitz, Rutgers University. Were there any subsystems that were delivered within specification but that eventually drifted out prior to launch?

MR. CHRISTY: I think we can say flatly "no," that they came in hot and they stayed hot, or they came in clean and they stayed clean. It's an overall statement. I don't recall any exceptions. There were only 4,000 or 5,000 pieces of hardware in this, so I don't remember.

VOICE: (Inaudible) from Lockheed. You mentioned you depermed the solar panels. What has been your general experience with the degaussing of equipment?

MR. MACLAY: That's our only experience with degaussing of equipment. I think that later in this program someone is going to describe an effort on degaussing other flight hardware.

CHAIRMAN GAUGLER: Just in case there are any old Navy men present, and I know there are, degaussing, in this space business, sometimes means deperming. I don't think anyone has done much real degaussing where you put compensating current loops.

MR. WOOLLEY: I would appreciate knowing what you mean by the term "deperming." I hear these terms "degaussing" and "deperming" being used all over the room, and I don't know for sure what you mean by that.

MR. MACLAY: What we mean by it is, by suitable black magic treatment you reduce the field of the unit to some suitably low value.

MR. WOOLLEY: You mean any method that you use, whether it's permanent magnet field or current-loop field, you call this either degaussing or deperming?

MR. MACLAY: Well, we use an AC field and reduce it slowly. This is the only technique that we have used. We've not used compensation.

CHAIRMAN GAUGLER: Strictly speaking from a Navy point of view, they should be talking about deperming all the time.

MR. BASTOW: I'd like to just add a comment here. On the Laboratory here we've gotten, or we started with, the habit of describing degaussing and deperming interchangeably. I finally have joined some of the others who are now calling it deperming, and tomorrow I will talk about the solar panel deperming. However, what we mean is the using of the AC field to distrub the domains in the materials so that they result in no perm field at our sensor. Now there is a big question about whether that stays that way, once we've done it. It's the same thing that the watchmaker does when you take your watch to him and have him stick it in a coil slowly and pull it out to make your watch run.

MR. GRUMET: Grumet of Republic Aviation. In connection with what you have just said, we have done some work in a shielded environment on Mumetal, and we found we could degauss and degauss completely; but the minute we removed the Mumetal out of the shielded environment and into the Earth's field, in a matter of seconds it gets remagnetized. Of course, these are very small fields. However, soft materials will get demagnetized readily, and harder materials need some tapping, but they all immediately on reinsertion into the Earth's field, do get remagnetized. And, I find it a little confusing to understand how you can deperm it with total loss of magnitude.

VOICE: Is this permanently magnetized or induced?

MR. GRUMET: No it's induced. We take Mumetal and degauss it by the technique you described, decrease the AC amplitude, and I do this in a 10^{-7} gauss shielded environment and take all the measurements. I can detect no measureable field in the Mumetal. I move it into the Earth's field and in a matter of 2 or 3 sec it's remagnetized and in the 10^{-7} shield environment I still measure some magnetic field.

CHAIRMAN GAUGLER: I think you have to be concerned about the demagnetization factor and the coercive force of the material. If you have a long Mumetal rod, it would saturate in the Earth's field; whereas, if you had a sphere, you would get very little. If you once demagnetized it, you would get very little.

What some of the people will talk about later on (Mr. Parsons and Mr. Iufer) will be where we'll deliberately perm up a spacecraft by putting it in maybe a 10- or 30-gauss field, just to see what its capability is. I mean, you can take an alnico magnet and heat it up over the Curie point, and you wouldn't even know it was magnetic. And still, if you start tapping it, rubbing it, bringing a voltohmmeter near it, or something like that, all of a sudden you find it has a big magnetic

moment. So, the people who (the avant garde section of this group) know about this will deliberately perm up the components, measure under that condition (which is many, many times what you would ever encounter on Earth), but you might encounter this on a shake table, for example, or you might encounter it if a man sets a meter or takes a screw driver, for example, and puts it on a certain component, you might get 10, 20, or 30 gauss. But they do that just to know what the capability is, and then they try to define the magnetic moment of the spacecraft to have a limited capability, not just take what you get and try to demagnetize.

But you're perfectly right. If you have a low coercive force material and a bad demagnetization factor, it'll do you no good at all to deperm.

MR. WEINBERGER: Weinberger of General Electric. I am wondering whether or not you're putting too stringent restrictions on the component design with this magnetic field requirement, because the net field is made up of the vector sum of all your miscellaneous components. Do you have any comment on that?

MR. MACLAY: I don't know a good way to take the spacecraft field and subdivide it and proportion it out to the subsystems today. If you did an algebraic division, I'm sure that it would be too stringent. We did not do that on the current Mariner. The spacecraft field that was hoped for, something like 1 gamma at the magnetometer location wasn't the number that we used to set the subsystem allocation.

MR. WEINBERGER: How do you allocate the subsystem as a ratio, do you use a rule of thumb or what?

MR. MACLAY: We didn't, I think we should; we didn't on the current Mariner. We knew the articles we would fly were the second generation of the same articles we flew on the Mariner Venus, which we had measured and knew the bulk of them were around 5 gamma. It was a different approach; we didn't try to subdivide.

MR. CASANI: I might elaborate on that point a little bit more, to the extent of pointing out that this restraint that Mr. Maclay has been talking about we applied to the subsystem level or assembly level, which generally consists of an assemblage of small subassemblies. The same requirement also applied to the subassembly level. We didn't attempt to scale down this 5 gamma at 3 ft to differentiate whether the test was being applied to a subassembly that is a 6- by 6-stack of electronics 1 inch or so wide, or to an entire assembly, consisting of perhaps 20 of these subassemblies. So I don't know if that contributes any to the question that you had.

There have been a couple of questions now that relate to measuring the magnetic fields up in the hills someplace, or here in the assembly building. What happens to it by the time you launch? I think it might be interesting if you could relate the experience we had with reestablishing the field after launch, and how it compared to the field that was measured before launch.

DR. SMITH: Actually, most of the experience that we had comes from the mapping of the subsystems. There are other people who did that mapping. Many of the units were mapped before and after the shake, and it was clear there was a very large effect associated with the environment that the subsystems had seen when they were on the shake table. As far as the spacecraft, an experimenter always wonders what sort of handling these things will get. It's not unusual to find people putting their subsystem in a steel cabinet, and you may find a bar magnet lying nearby. As I have said, I think with the variety of different environments in some cases the fields associated can be very large.

Now, about the only way that I can try to answer what I understand is, Mr. Casani's question is to compare the measurements that were made on the different spacecraft.

MR. CASANI: I was thinking of the measurements that we made in flight by the rolling of the spacecraft and how the determination of the two axes were made at that time, compared with the measurements of the field components along those axes on the ground.

DR. SMITH: This was done in the case of both Mariner II and Mariner IV. On Mariner II there was a very great discrepancy, something on the order of 50 gamma, between these two components perpendicular to the roll axes as they were measured on the ground and as they were measured in space. Now, at that time, there was some thought that maybe there had been a mistake made in the mapping of the spacecraft; where we determined the fields at the sensor contributed by the permanent magnets and the permanent magnetic material in the spacecraft. We took one of the spare spacecraft to Malibu and put it in a zero facility and got essentially the same result we had gotten by rotating the spacecraft around two axes in the Earth's field. This was a very large change, and as I have said, about the best that we could ever make out of it was that it was somehow associated with current loops. Incidentally, current loops don't yield very well to deperming.

Now the situation is somewhat different on Mariner IV. Again we determined the two components in space, and they agreed quite well with the measurements that

were made before launch. By "well," I am talking about something on the order of perhaps 2 gamma on each axis. As I tried to point out in my talk, I think when one takes a realistic view of how you determine the spacecraft field before launch, it is very difficult to believe that you are going to do that to a fraction of a gamma when you have to determine the fields because of the various contributors, in a piecemeal way. We determined the contribution from the permanent materials in one test; we determined the current loop fields in another. The permanent fields of the solar panels and the current loops associated with the solar panels are measured separately.

Now, this is a part of trying to deal with a very large spacecraft. Mariner Mars is very large. I think this afternoon someone will show you a picture of the mapping fixture that was used to determine the permanent fields of the Mariner Mars, and it is a very large structure. It just seemed impractical, in view of all the constraints, to try and rotate the spacecraft with the full solar panels on. As far as we know, though, on Mariner IV (within the limitations of our measurements) the fields on those two axes in space look very much like they did before launch.

I also might make a comment about this, comparing the different spacecraft. There were four, I believe, spacecraft mapped during the Mariner Mars program. They tended to look pretty much the same. Mr. Bastow mentioned the contributors, and it seems clear to me (this is an opinion, really) that what we are seeing is the superposition of a very large number of fields. If you total up the number of fields, the number of subsystems that will contribute gamma at the magnetometer, it represents a substantial fraction of the entire spacecraft.

Once this spacecraft was subjected to a presumably common environment, like putting it on a shaker, the fields tended to be very nearly alike within, say 5 gamma or so. The fields tended to be strongest along the Z-axis at magnitude somewhere around 30 gamma; so this seemed to be something that repeated within 5 or so gamma from spacecraft to spacecraft, and then was seen again in space.

CHAIRMAN GAUGLER: I presume that was a nonmagnetic shaker you were talking about? Some of these shakers have 30-gauss fields associated with them.

MR. MACLAY: Although we are sort of enthusiastic here about deperming, in our saner moments we still regard deperming as opening the door to Pandora's box. It is not the way to get a clean spacecraft.

CHAIRMAN GAUGLER: That is right; particularly in the case of Mariner Mars, for example. If you had had a perm test where you perm the spacecraft up

to 30 gauss — I can just imagine getting Jack James to be willing to do this 6 months before launch — but, if you had the time to do this, of course, you could do it. Then you would find that the field you would get in the permed condition would be something gigantic, probably a 1,000 gamma or something like that, 10,000 gamma, maybe. The only way you can really seriously approach this problem is to define the perm value of the spacecraft under the maximum condition you think it is ever going to get exposed. Once you do that, then you have some hopes of deperming. Otherwise, it is just opposing two very strong forces and hoping that the net result is zero.

MR. PARSONS: Lee Parsons, Goddard Space Flight Center. We have done a fair amount in the deperming of some of these satellites and spacecraft. A great deal hinges, of course, on what's inside the black box. But we find, in general, you can accomplish (when the job is done and after the craft is getting closer to launch and there's still too much field to satisfy the experimenter flying aboard) deperming. It is one of the few things left to do, and we had to do it; however, our indication is that it is fairly stable in a depermed state. The results of the last IMP flight would indicate that the perm level never came above that which we had measured in our previous prelaunch test; namely, 1/2 gamma at the position of the flight detector.

I am quoting these from the experimenters' analysis of his flight data. I wasn't there, so I am not sure. But from what they saw of it, it depermed out. The OGO, of course, has also had some deperm treatment. It is not the cure; it is not the way to build a nonmagnetic spacecraft in the first place. But it sure is a help when you get down to the point where something has to be done.

Now, a while ago, there was a question of computing from the component data — a net value to be expected from all sources aboard at the flight detector position.

We have attempted this also; not with very much success, I am afraid especially insofar as the larger types such as the OGO. It has been difficult to get all the data together, and it has been difficult to work with. We haven't succeeded too well there, although we had at least a ball park figure — an upper and lower limit, that we expected. On the IMP, just recently, on the very latest set of hardware, we have again tried it, this time with a little more success. We think in that small a craft where hardware is treated in a similar manner as it goes through, we think perhaps we might be within the order of 20 or 30%. This was not true of OGO, however.

DR. SMITH: I wanted to make one comment that perhaps wasn't clear. It has to do with the stability of the deperming. I should have said this when I made my earlier remark. Before the solar panels were depermed, we were expecting a field at the sensor on the order of 100 gamma becuase of the solar panels themselves. This was due to the bus bars that were used on the solar panels. And, as I have said, after they were then depermed, essentially, the perm contribution just dropped out of sight. Afterwards we found that the measured fields in space on the two axes are within a couple of gamma of what we measured before launch, irrespective of the panels; which indicates to us that the panels did stay stable once they were depermed.

CHAIRMAN GAUGLER: I think some R & D ought to be done on this, but I think that what helped you there was this coercive force. In other words, although you like everything to be nonmagnetic, having a high coercive force (which the bus bars would have), probably 50 gauss or something like that (maybe more), would probably help you. If it were a piece of Mumetal in that case, and if it had the wrong demagnetization factor (which it did) then it would have been much more serious.

DR. SMITH: This whole area of stability needs a lot more work. It could really stand a lot of R&D.

MR. NOBLES: Nobles, Applied Physics Laboratory. I don't quite understand why this perming and deperming has arisen. It seems to me that the old terms of magetizing and demagnetizing are much clearer and more appropriate.

CHAIRMAN GAUGLER: I think you are absolutely right. I think this sort of came up from the Navy when you talked about the perm in a ship; and degaussing, of course, is being misused here from the Navy point of view. I think you are absolutely right, but whether we can get people to do this or not is something else.

MR. WEINBERGER: I think it does have one value, and that is it distinguishes between the induced magnetism, which you can't demagnetize, and the perm.

CHAIRMAN GAUGLER: That is right. Of course, you are talking about the fact that a ship would have a certain perm, like a permanent magnet; and then you would have an induced perm that, as it rotated around the Earth's field, it would induce it in. Is that what you are talking about?

MR. WOOLLEY: Every time I hear this term "perm" I am puzzled. I wonder if you mean permanent magnet. Every time I hear the term "perm" I find myself

wondering, now does he mean magnetize, and each time somebody tries to explain it they confuse me more, because they use more jargon terms. Now, if I am the only one who doesn't know these terms, I can't expect any change, of course. But I sure would appreciate some clarification.

MR. IUFER: It may sound like it, but I am not intending to defend the Navy's position on this. I think perhaps one way to visualize this would be to view the hysteresis curve.

At Ames we are thinking of this as several different processes. We are reserving the term "degaussing" for the technique, where the induced and possibly permanent magnetizations of a specimen are compensated by actively energizing coils of wire. When the current is off, the compensation is off, and you have the original field again. This term is in extensive use. Many of the people now working for NASA were trained in the Naval degaussing program, and it is hard to avoid old associations.

Now let's discuss permanent magnetizations; perming and deperming. The word "perm" originally developed as a jargon or shop talk term for permanent magnetization. "Perm" is also called remanence or residual induction. A sample is exposed to some field, the field is removed, the sample has remembered this exposure by its remanence. We call this value the "perm." It corresponds to the cross-over of the curve, at B_r where H is zero on the retrace.

There is also an induced magnetization, which for the geometries of hardware that we have measured is proportional to field values up to several times higher than Earth's ambient field. We have also found on actual flight hardware that the remanence value is usually proportional to the exposure field when the exposure field has not exceeded 25 gauss. This means, by virtue of the large demagnetization factor, that the hysteresis loop is sheared in a clockwise manner, so that the knee of the magnetization curve — instead of occurring at perhaps 10 to 20 gauss — may actually occur at 100 or 1000 gauss.

Deperming is a process where magnetic remanence is removed. This is normally done by running the material through hysteresis loops of diminishing area until you arrive at a point at the origin of the hysteresis curve. The tips of the loops represent the normal magnetization curve that you are retracing back down to the origin. Later on I will discuss some of our findings in simplified techniques of effectively deperming spacecraft hardware.

MR. WOOLLEY: Take a phrase like "perm up to 30 gauss." By this do you mean subject to a magnetic field of intensity of 30 gauss, or does it mean something completely different?

MR. IUFER: It means essentially it is exposed to 30 gauss.

VOICE: Have there been any attempts to install permanent magnets to null out the residual field of the spacecraft? Do you think this might be a fruitful technique?

MR. MACLAY: The answer to the first question is no. If you are concerned with stability, your compensating magnet may be stable – very likely your spacecraft field that you are trying to get rid of is not stable. If it is not stable, you will end up with perhaps the same problem you started with, and now you are flying more weight.

VOICE: On Mariner IV, there apparently was a more or less stable field of about 30 gamma.

MR. MACLAY: We didn't have any prior assurance that it would be stable. I believe that what Dr. Smith described on the Mariner II indicated it wasn't stable at all.

DR. SMITH: I have a point of view about this too. If you have a stable field, and you know it is 30 gamma, 30.2 gamma, what point is there in trying to compensate it?

MR. PARSONS: In answer to that very last question, there is, in some cases, a problem of computer programming, that is greatly simplified. This remanent perm, proven to be stable, can be compensated by a "stable" magnet. This is being considered seriously in one program, at least. In the case that hasn't received much attention so far: the problem of attitude control of a spin-stabilized spacecraft in the Earth's field; where there is a prominent moment (that is found to be stable by experiment, by attempting to deperm it and failing) it has been compensated with an Alnico magnet, in fact two magnets. For the United Kingdom UK-I and UK-II satellites this attempt was made and I believe it will be tried again, in the near future. Truly you have got to have a stationary permanent field to compensate with the permanent magnet; but it should be possible, and might be the only way out in some cases.

VOICE: Isn't it also true that some of the experiments have a saturation level, where if you have a permanent field they might not be able to measure variations about that field.

MR. MACLAY: Yes, that is true. On Mariner Mars the spacecraft field was below that saturation point. They have another point that isn't even the saturation point, and this is a point where they changed scales. We were below that scale change point also. The Mariner Venus had a scale change.

VOICE: I don't know if this will add to the confusion or make a clarification. But I believe, if a virgin material is subjected to a magnetic field, the value of the flux density it is left with is called residual induction; however, if it is subjected to a cyclic field, the value of the flux density that it is left with is called remanence.

MR. KELLER: Keller, Goddard Space Flight Center. Has anyone done any work with degaussing coils on subsystems where they are not powered until the subsystem is turned on?

CHAIRMAN GAUGLER: This is real degaussing now.

MR. MACLAY: I am not aware of any work like this.

MR. PARSONS: I think that last question back there is what I would call stray field compensation rather than active degaussing coil work. It is a condition where there is a field generated only when a current flows within the experiment package. This, if surrounded with a loop of wire carrying the same current in the opposite direction, sometimes can be balanced to compensate this. This has been done in several parts of the IMP hardware; I won't attempt to bring to mind right now. I have all the data with me though if anybody wants to go into it. They have been fairly successful getting things like 80 to 90% reduction in stray field from one package by inserting in it a compensation loop. Again I don't call this a degaussing coil. This is a stray field compensation.

MR. MOSKOWITZ: I would just like to mention that on the Tiros program, this was a dipole moment we were compensating for. We did use permanent magnets. Also, we used permanent magnets on the Relay program – this is at RCA. We flew a University of Iowa magnetometer package, and we had a requirement on that of steady field and AC field at the magnetometer location. For the DC field on one

Relay satellite, we found that we were excessive, and we used a small permanent magnet to cancel it out or to reduce the field at that particular location to a reasonable level.

MR. WOOLLEY: We have used both of these techniques quite successfully on the OSO. Both permanent magnets and wire routing. After all, the wire routing is (you can call this a degaussing coil, certainly) the same difference, but both of these are used on the OSO with success.

N66-11280

MAGNETIC PROGRAM FOR PIONEER AND ITS SCIENTIFIC PAYLOAD

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In this presentation we are going to discuss the mission of the Pioneer program. We will give you a general description of Pioneer so that you can relate it to other spacecraft programs. We will try to give you an idea of the organization of the magnetics management for Pioneer. We will be considering the basic factors establishing the spacecraft specification.

We will be discussing the spacecraft acceptance criteria, the payload acceptance criteria, spacecraft test plan, payload test plan, and our present prediction of the spacecraft field with and without the scientific payload.

The purpose of Pioneer is to make interplanetary measurements. It has no other mission. The magnetic field exploration has priority one and, therefore, the magnetic properties of the spacecraft, of course, are very important to this program. In Pioneer, we've tried to use the experience, both good and bad, that you have heard already today. Pioneer has not yet flown, it will fly later on this fall. Design began approximately 2 yr ago.

Because the magnetic mission was so important to Pioneer, and because we were able to establish firm ground rules for the fabrication of Pioneer at the beginning of the program, we were given essentially a fortunate set of ground rules that permitted us to put teeth behind specifications. In Pioneer, the magnetic guidelines were not suggestions, they were mandates. The guidelines for construction were a contractual condition for the experimenters. And the magnetic fields permitted on the spacecraft were part of the acceptance of the spacecraft; bearing the same weight as power, and other very important criteria.

In this brief talk, we will be limited to general concepts. I find that I have been put on the program on two other occasions, in which case there will be opportunity to go into detail of the whys and hows of construction.

Pioneer is designed to go into sun orbit. It has essentially two types of orbits: one, to be at a mean radius of 0.8 AU, and the other 1.2 AU. It is designed to have a life of 6 mo in orbit. It is designed to transmit scientific and engineering measurements until it reaches the communication range of 50 million mi. In Fig. 1 we have a model of the spacecraft. The three booms are mounted near the equator. One boom

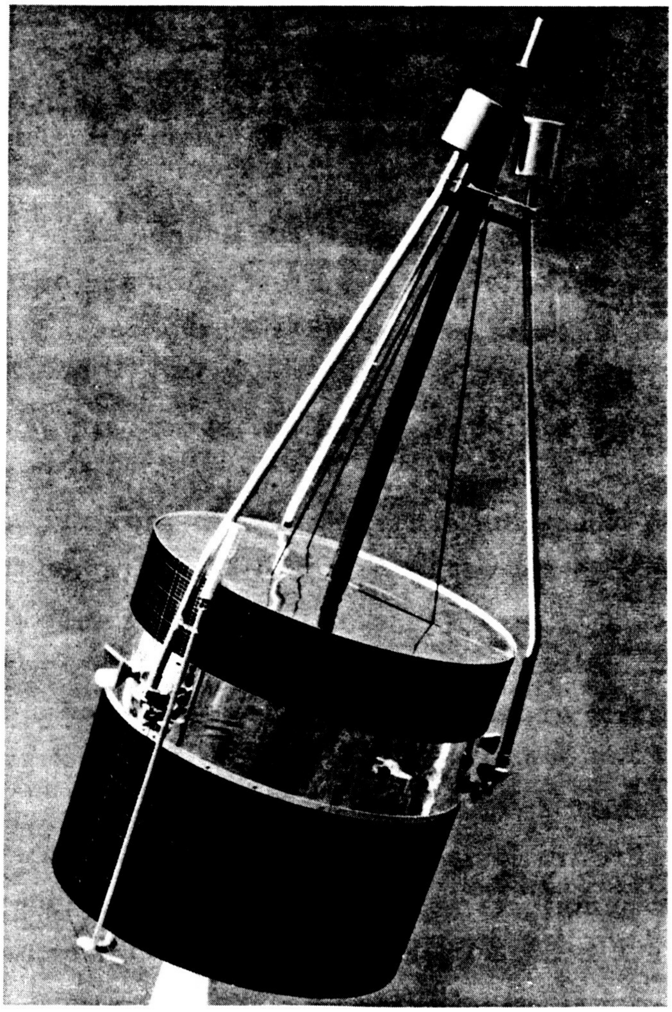


Fig. 1. Pioneer spacecraft model

supports the sensor for the flight magnetometer, a second supports the gas nozzle for orientation, and the third is for the mechanical damper. A small boom hinged to the bottom of the spacecraft is an antenna for the Stanford Radio Propagation Experiment. The flight magnetometer is contained in the cylindrical can to the left of the figure. The overall length of this boom is 82.44 in. The actual distance of the magnetometer transducer is approximately 80 in. from the cg of the spacecraft. The spacecraft itself, is about 3 ft in diameter and about 3 ft high.

Pioneer weights approximately 140 lb. Of this, approximately 30 lb are scientific instruments and 50 lb are engineering instruments (Fig. 2).

Thermal control is brought about by the louvers in the aft end of the spacecraft. These louvers are controlled by bimetal springs. As the temperature inside the spacecraft rises, the louvers open and permit heat loss through radiation.

The spacecraft is designed to be spin-stabilized. The spin-axis will be approximately normal to the plane of the orbit. It will rotate approximately 60 rpm. This will permit rather uniform heating of the spacecraft, a property that we will refer to when we talk about stability of magnetic remanence.

A layout of the shelf is shown in Fig. 3. Approximately 25 black boxes are required for engineering functions and eight for scientific functions. Most of the scientific experiments have viewing requirements so they must be mounted on the perimeter of the shelf. These boxes range in weight from 3 to 5 lb each. Pioneer is a rather complicated spacecraft. For example. it is designed to implement some 57 different commands by switch closures.

As you may recall, not too long ago formal reliability control was something new being introduced into spacecraft programs. At first it met with considerable resistance by managers and designers alike. There seems to be a natural reluctance to adopt new ideas or new restraints until it has been demonstrated that they result in something worthwhile. This initial resistance to new restraints is being keenly felt by programs designed to produce magnetically clean spacecraft. To achieve a successful magnetics control program, one must start with effective indoctrination. In the development of space hardware, there are usually a lot of votes taken before major decisions are made. Someone primarily interested in meeting the magnetic cleanliness objective must cast his vote with authority equal to the others. Otherwise magnetics will fall to lowest priority.

On the Pioneer program, the magnetic portion of the mission has first priority. To assure that the spacecraft was built in a manner compatible with this mission, a

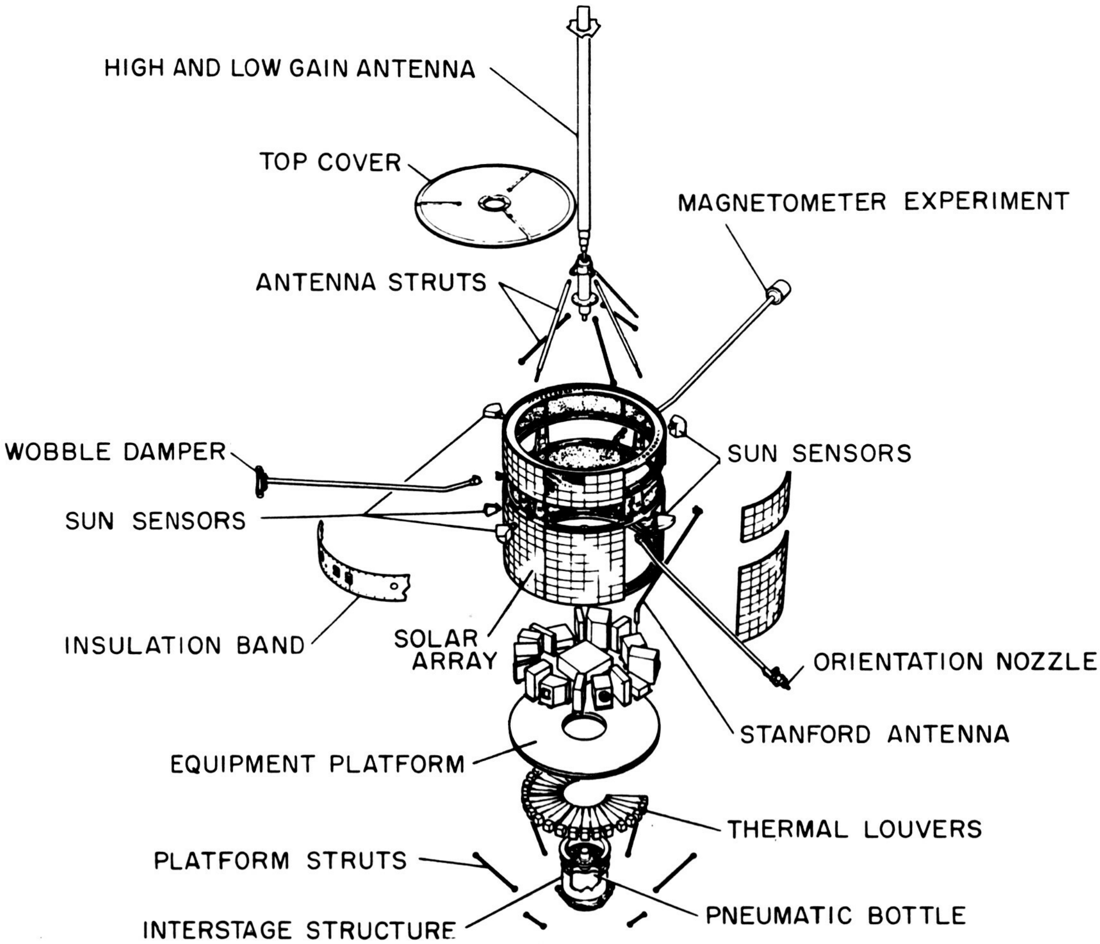


Fig. 2. Pioneer spacecraft equipment

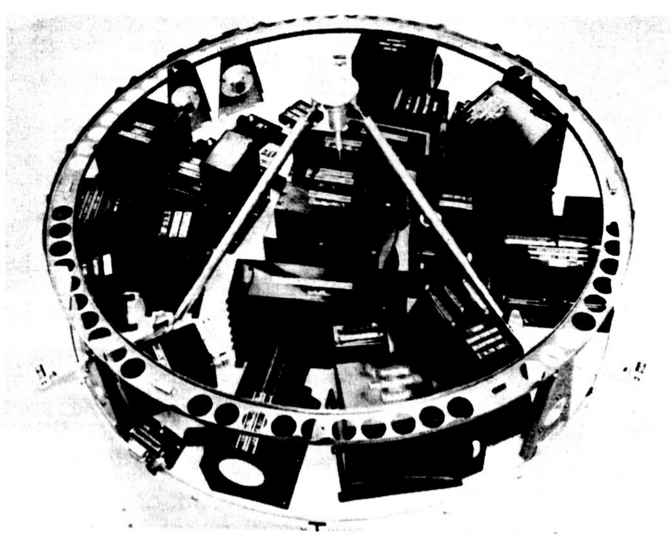


Fig. 3. Platform layout of electronic assemblies before micrometeoriod detector deletion

position of Manager of Magnetics was created in the NASA Project Office. The individual occupying this position was given responsibility for acceptance and test specifications, test procedures, and that the hardware would meet these specifications. By his being parallel in staffing to the Spacecraft Manager, the Experiments Manager was in position to see that proper attention was given to the magnetic characteristics. Pioneer is being built not in house at Ames, but by a contractor in Los Angeles. The same policy of adequate administrative authority has also been followed by the contractor. This contractor has an Assistant Director for Experiments and Magnetic Control who reports directly to the General Program Manager. The particular Assistant Director for Pioneer happens to be a senior staff member and has had personal experience as an experimenter. By his background, he is well qualified to champion the interests of this particular program. He is assisted by a Subprogram Manager for Magnetics Control who is responsible that proper specifications are written at all levels of design and fabrication, and that the test plans used to determine the magnetic properties of the boxes are valid and properly applied.

I would now like to discuss the factors establishing the spacecraft and magnetic specifications. There are essentially four that I would like to review at this time. The first one concerns the maximum permissible boom length. If it were possible to launch the magnetometer sensor a day after the spacecraft it would not be necessary to have any magnetic restraint on the construction of the spacecraft. This, of course, is obviously impractical. So, one must make compromises and, when, in some cases the sensor is closer than 40 or 50 ft, it is necessary to start compromising.

On Pioneer, the maximum practical boom length was found to be 82.44 in. The boom length was limited by the requirement that it fold inside the shield or fairing with a single hinge at its base. Deployment is brought about by ordnance cord cutters and centrifugal force.

Along with boom length, one must consider the level of the fields one is trying to measure in space. As you heard, this level is nominally 5 gamma during the quiet sun period. Consequently, the uncertainty because of spacecraft signals or noise should be significantly less than this 5-gamma level.

Another factor in establishing the magnetic specification is the state of the art in nonmagnetic spacecraft design. It is pointless to set a specification so difficult to achieve that people will refuse to try, or be reluctant to cooperate. In establishing the magnetic specification for Pioneer we found out what had been achieved in previous

programs and, then, tightened the specification to the point where we thought we had a possible goal consistent with the mission objective.

The fourth factor used to establish the spacecraft magnetic specification was the probable effects of launch. Bearing in mind that the interplanetary field being in the order of 5 gamma, it is evident that the fields resulting from the ambient magnetic field, or so-called induction field, are going to be negligible. Consequently, we will only be interested in the permanent magnetizations of the spacecraft, plus the stray fields that are produced by energized circuitry.

To assess what levels of remnant fields one can tolerate, it is important that one have some knowledge of what would happen to the spacecraft if it were first completely demagnetized by deperming, and then launched in Earth's ambient field; where you would have an external magnetic field in the order of 1/2 gauss plus the various types of vibration because of launching acting on the spacecraft. In this area, we have developed a rule of thumb that seems to be somewhat consistent with experience to date, although no scientific experiments have been conducted that truly measure just the effects of launch.

This rule of thumb is if the spacecraft were completely demagnetized and then launched, you would get a result that is comparable to an exposure of 5 gauss DC. Perhaps I could illustrate this by saying that if one launches a demagnetized spacecraft one would get the same effect as if a demagnetized spacecraft were put in a zero field, mementarily exposed to 5 gauss, and then measured for the resulting change in remanence.

One other complication is the ability of a spacecraft to become magnetized is not equal for all axes. We feel that the 5 gauss probably represents a worse case condition.

The next area that I would like to discuss is the spacecraft magnetic acceptance criteria. This is essentially a specification to which the contractor must build a spacecraft for it to be accepted. This criteria defines the field allowance at the magnetometer. It does not refer to the equivalent dipole moment of the spacecraft or fields on the surface of a sphere. It only refers to the background field that the magnetometer will see in flight. The spacecraft contractor is not responsible for the magnetic field contribution of the scientific payload. Therefore, the NASA Project Office had to allocate part of the total permi ible magnetic field to the spacecraft and part to the scientific instruments.

The Pioneer specification permits a stray field level of 0.5 gamma at the magnetometer from DC through 25 cps. We say DC because we are also interested in the stray fields produced by DC power circuitry, not only alternating fields. The criteria for acceptance after a deperming treatment is 1 gamma at the magnetometer. And the third criteria permits a 16 gamma field at the magnetometer after a 25-1 gauss DC exposure parallel to the spin axis.

By the magnetometer data received from spin stabilized spacecraft, it is possible (in some cases) to determine the radial component of spacecraft field after launch. This permits the tolerance of radial field to be less stringent than the tolerance for field components parallel to the spin axis.

A spacecraft field component parallel to the spin axis cannot be determined on the basis of flight magnetometer data unless one performs a maneuver where the spacecraft is tumbled about an axis other than the spin axis. Now, if one were able to in-flight measure continuously all the fields aboard a spacecraft, the requirements for the magnetically clean spacecraft could be essentially eliminated.

It was not possible to change the spin axis on Pioneer, but we have relaxed the specifications for the radial field, and have kept a tight specification for the spin axis field component.

Now, I'll talk a little bit about the scientific payload acceptance criteria. There are six experimenters on board Pioneer. Each experimenter was given an identical field allowance. This essentially defined a steady-state field after a 25-1 gauss exposure of 2 gamma at 3 ft. We permitted a stray field level of 0.5 gamma for the range of 0 to 25 cycles.

It is important to note that unless one establishes not only the field magnitude permissible, but the range at which the measurement is made plus (in fact) the orientation at which the field exposure was made, one does not have an unequivocal specification. We will be talking about some of the types of field exposures later in another paper I am going to give.

At this point, I will say that for the experiments, the acceptance test goes something like this: The experiment is measured for the "as-received" magnetizations, then it is depermed in a 25-gauss field, and then it is remeasured in a coil facility where Earth's field has been very significantly reduced. We find that the reduction necessary is about 2 orders of magneitude for definitive results. If the experiment after this demagnetization process does not go below 1/2 gamma at 3 ft, we find out why.

In many cases, we have found the reason that the 25-gauss deperming did not accomplish what we had hoped is because the parts or assemblies in the instrument have been exposed to a field that is much higher than 25 gauss. On individual parts, we have found that they have been exposed to fields in excess of 800 gauss and that it was necessary for us to apply fields of several hundred gauss to eliminate this magnetic history. This has become one of the ground-rule precautions. Do not check items for their magnetic properties with permanent magnets. If you measure the face of a permanent magnet, you get values all the way up to 1200 gauss. In some cases, it may be a hazard to the device to expose it to fields to remove this high an exposure.

The scientific instrument is then exposed to 25 gauss and then redepermed along three orthogonal axes. The resulting remanence is measured at 3 ft following each exposure.

The magnetic test plan for the complete Pioneer spacecraft is as follows. The prototype spacecraft will be measured as a complete unit just before qualification. The purpose of this is to determine whether or not the magnetic criteria has been met for the boxes that are then assembled. If, for any reason, a box is found to be very magnetic, and consequently would have to be reworked, we would want to know this information before we had qualified it aboard the spacecraft. So, it is very important to know how one stands as early as possible in the program.

All flight spacecraft will be measured once just before shipment, and they will be depermed just prior to shipment. The steady-state fields in each general spacecraft measurement will be mapped. This means they will be measured at various points on the surface of a sphere and they will be measured during the course of tumbling the spacecraft on a gimbal system. The stray fields will be measured along a radial that essentially intersects the flight magnetometer. We do not make one-point measurements at the flight magnetometer because, in some cases, noise level predominates over other uncertainties and, therefore, to measure nearer the acceptance point, can yield useful information.

The deperming process on Pioneer will start with an exposure of 50 gauss and it will be given subsequent exposures using a sinusoid having a frequency of approximately 1 cps. The standardizing exposure is specified as 25 gauss. For the flight units this exposure will be given only parallel to the spin axis. For the prototypes, we would like additional scientific information to establish how well we can predict fields. Consequently, the prototype spacecraft will be depermed and exposed along each of the three spacecraft axes.

As you've already heard today, Mariner fields were nominally about 30 gamma and because of a ground loop they went higher. We understand that the IMP spacecraft, which was very successful, had a value of approximately 16 gamma at the magnetometer after a 25-gauss exposure with something less than 1 gamma after deperming. On Pioneer we will not have a complete spacecraft measurement until later next month. We have measured all the prototype black boxes that, I understand, some centers call proof test models. We also have measured many of the first flight units of hardware. At Ames, the scientific payload has been mapped as individual items. They have also been assembled on a mockup of the instrument shelf and then measured as a group.

On the basis of these measurements, it is predicted that the scientific payload will contribute approximately 0.2 gamma at the sensor. This corresponds to a magnetic moment in the centimeter-gram-second (cgs) system of 8.4 dyne-cm/gauss for a dipole equivalent. In the permed condition, the contribution will be approximately 1.4 gamma, which corresponds to a magnetic moment of approximately 59 cgs units.

The rest of the spacecraft is now predicted to be, in the permed condition after 25-gauss exposure, 1.4 gamma, for a dipole equivalent moment of 59 cgs units; and the depermed spacecraft, 0.6 gamma at the sensor for a magnetic moment of 25 cgs units.

We want to point out that these are best estimates to date and we, naturally, are still looking to the first overall spacecraft tests that will expose any sins if some have been committed.

I might make a little comment: the ratio between permed and depermed is less for the spacecraft than for the scientific payload. There is a reason for this. Because of the communication range of Pioneer, it is necessary to use traveling-wave tubes. As you know, traveling-wave tubes use magnets over practically their whole length. These magnets are very stable, they are made from a platinum cobalt alloy. But it is still very difficult to eliminate their leakage fields entirely. Their leakage fields have been reduced by compensation.

To develop a compensation system that is going to track the leakage field of the magnet, the compensation system must have the same temperature and aging properties. With this in mind, more of the same material is used; namely, platinum cobalt magnet material.

In conclusion, I would like to review the deperming criteria; because of some of the questions that were asked earlier in the day. It's our experience that a high

DC magnetic field is not injurious to flight hardware. You will find that some of the manufacturers of semiconductors actually use magnets to handle their components. We discovered this when we were mapping some of these parts.

It is best to do your deperming at the lowest system level. At the screening inspection, it would be good to do your deperming because you now require a very simple fixture, essentially a growler (which is a device used in armature repair shops). It can be used to produce fields up to 1000 gauss at 60 cycles. This does a splendid job of deperming recalcitrant specimens.

One must be careful of using powerful AC fields on assemblies, because they can induce currents that may exceed the ratings of some of the parts, particularly the junctions of low-level transistors. The theoretical lower limit in deperming seems to correspond to the remanence or residual induction in the specimen by the 1-gauss level of Earth's field. This corresponds to approximately 2% of the 25-gauss exposure we use.

It is rather pointless to refine a deperming technique that removes a memory or remnant magnetization of a sample well below Earth's field when it will just reacquire this magnetization when it is transported in Earth's field. We find that the efficiency of the deperming operation is essentially based on the level of the first shot. One must reach a level that is comparable to the level at which the specimen was initially exposed. In other words, you can't use a 10-gauss field to remove the magnetization produced by a 100-gauss field.

We find that the coercivity of flight instruments on Pioneer is such that a 25-gauss field will, indeed, reverse a 50-gauss field. This means in terms of the B-H curve that if you expose a sample to a 50-gauss field and, then, reverse your polarity and expose it to a 25-gauss field, you wind up with a demagnetized specimen. One has a two-to-one advantage over the previous exposure when it comes to deperming. As a consequence of this two-to-one rule, one can get errors in measurements of remanence comparable to 4% if one transports the specimen through Earth's field between the exposure equipment and the magnetic mapping equipment.

It also points out that if one adopts a standard exposure, it should be sufficiently high so that the correction because of the exposure to Earth's field can be ignored or easily corrected for.

OPEN DISCUSSION

VOICE: In the program here, there is a "development of nonmagnetic parts list," we have asked for that before, and you seem to have one here.

MR. IUFER: Yes, on the third day of this symposium, I will be giving a paper on parts and materials, and we will be discussing how some of these lists are adopted. There is a precaution here; not everyone's requirements are the same. So, a program not requiring the magnetic cleanliness of a Pioneer may not want to pay the price for Pioneer parts; however, all the experiments and parts used on Pioneer have been incorporated into a list and it should be available on request from the Pioneer Project Office at Ames.

VOICE: Do I understand that in the traveling-wave tube you used, you added magnets to compensate this? External magnets to the tube?

MR. IUFER: Yes.

VOICE: Then, I don't quite understand the basic philosophy that I understood this morning, in the sense that we should not use magnets. In this case, it is permissible, apparently. What is the reason here?

MR. IUFER: Well, the policy in general is to try to avoid introduction of the field in the first place. It is not possible to design a traveling-wave tube without magnets, so, you have them. What do you do about it?

VOICE: If you need them, and you can't do anything else, you must use them.

Mr. IUFER: Right.

CHAIRMAN GAUGLER: I think that in the case of a magnet that has been deliberately put in there, it probably has a high stability. But, when you have something that gets in there by accident, like somebody rubs a screw driver along part of the spacecraft, well, I think that tends to be more unstable. Isn't that sort of the philosophy?

MR. IUFER: Right. In this case we knew what the stability of the source was and it is easier designing a compensating method using something of comparable stability. But, ordinary spacecraft remanences are not stable, they can change several percent just in Earth's field exposure. If you are staring out with a 5000-gauss magnet, and it's contribution is 100 gamma at the magnetometer, these percents are intolerable.

CHAIRMAN GAUGLER: I think all this stability is related to aging and coercive force. This is a question that always comes up: How stable is a magnet. One-half % meters, for example, have magnets in them. The fact that they remain a 1/2% meter over a long period of time if you don't use them, shows how stable magnets are.

VOICE: On the acceptance criteria for the spacecraft you mentioned 0.5 gamma, DC to 25 cycles, and 1 gamma after deperm. Does that mean that the component exclusive of the stray field is 1 gamma and that you can add onto that another 0.5 gamma when you put on any DC equipment?

MR. IUFER: That's correct.

CHAIRMAN GAUGLER: I think there is a unique problem in the case of Pioneer. It is a ceiling-type incentive contract where the contract was let before the experiments were selected and NASA goes through a very democratic way of selecting experiments. So, they had to have an interface between the experiments and the spacecraft, and this produces a lot of headaches because lots of times you can do things more simply if you can just treat it as a whole. So, we have this arbitrary interface and that's what he's referring to.

VOICE: Where do you put these little magnets? I presume you use a TWT (traveling-wave tube) structure?

MR. IUFER: Right on the end of the TWT magnet itself. They're actually a part of the TWT.

VOICE: You put compensating magnets – you had them built in?

MR. IUFER: Yes.

MR. SCHNEYER: Mr. Schneyer, Lockheed. I am interested in your experience on how close your prototype and your final hardware came. What sort of percentage differences did you experience?

MR. IUFER: Well, our experience has been in a favorable direction. On the experiments, one of the experimenters came in with a preprototype model that went by the name of design verification unit, and the first magnetic field measurement was 18 gamma at 3 ft after a 25-gauss exposure. We disassembled the instrument and found out what parts were causing the difficulty, we recommended some design changes, and when the flight unit came, this experimenter had reduced his field from

18 gamma down to 2-1/2 gamma at 3 ft. So, the trend has been that they are getting better, not that they are getting worse. In handling, on occasion we had a chance to measure one experiment before it was shipped back to the experimenter for calibration and test, then it was returned to us and we mapped the experiment to check what had transpired, and we found that the change was less than 10% of our 25-gauss exposure. So the handling of an experiment does not significantly alter the permanent magnetization of it.

MR. SCHNEYER: I guess I was interested in the correspondence between your first production piece, which you call the qualification model, and the first flight hardware, which were essentially identical and not redesigned. How close do they come together?

MR. IUFER: I'm not sure we have this information because the magnetic control program is not static and improvements are being made all the time. So, we actually do not have two models that are carbon copies for magnetic testing. Perhaps, the spacecraft contractor has that information.

MR. SANDERS: We have a prototype unit that goes on a prototype spacecraft. Then we have a flight unit that goes on a flight spacecraft, and we have what we call an engineering model. The engineering model was dirty, we had something like 200 gamma at 1 ft. And the reason for this, we couldn't get the nonmagnetic parts for it. And all we could use the engineering model for was to check the stray fields. When we got to the prototype unit — this unit is essentially the same as the flight unit and there is no difference between, so far — we measured very little difference between the prototype and the flight spacecraft.

MR. IUFER: I would like to point out that we are able to get these consistent results because we do not include the magnetic history of a specimen when we measure it. It starts out being depermed and it is given a standardized exposure, and then it is measured.

CHAIRMAN GAUGLER: I think that's a very important point, because if somebody takes a screw driver and rubs it over the surface, it will vary all over the place.

MR. BERGER: Mr. Berger of Lockheed. What effect does the deperming have on the equipment that contains permanent magnets like a TWT?

MR. IUFER: As I understand, the operation of the TWT, the performance of the ma et, or the degraded performance of the magnet can be evaluated by the increase in helix current. We find that deperming to date has not significantly increased this helix current. We've concluded that it is safe to expose these assemblies to 25-gauss DC exposures and 50-gauss AC exposures that are specified for the spacecraft.

MR. BERGER: Is this because the 50 gauss is so much less compared to the strength of the magnet itself?

MR. IUFER: Well, it is a matter of applying the field across the gap of the magnets. You just do not have a good coupling efficiency, and so you are not influencing the magnetic properties of the magnet as you would with a big bar magnet when you apply a longitudinal field to it.

MR. HUBBARD: Hubbard, Naval Ordnance Laboratory. I might point out on this permanent magnet thing that good, high quality, high stability magnets are knocked down anyhow, and so they are highly stable against extraneous fields to begin with. The manufacturer purposely demagnetized them slightly to increase their stability.

MR. TOSSMAN: Barry Tossman, Applied Physics Laboratory. When you apply a 25- and 50-gauss field to the assemblies, are there any preferred direction of the fields?

MR. IUFER: Very definitely so. This is because anything that's not spherical and completely isotropic, will have preferential directions of magnetization. On the experiments this is why we give them tests for exposures along all three axes. It is possible to design equipment so that you can get a preferential axis of 10-to-1, for example, if the remanence from an exposure in one axis could be 10 times higher than another axis. We have attempted to apply this, somewhat, on the Pioneer by constraining the field component parallel to spin axis. This field component cannot be evaluated in space and, therefore, we would like to suppress it as far as we can.

MR. PARSONS: Just another word in defense of deperming. I would like to say that we have depermed many satellites, beginning originally with the 20-gauss level working up to the 50-gauss level, and to the best of my knowledge, there has not been evidence of any failures, damage, or harm done to any of the payloads that we have depermed. Generally, we have also investigated particular pieces of

equipment such as latching relays, tuning forks, etc. In those, we found that we could go at least to 400 gauss before we could detect an operation range that was disturbing to the people who were the sponsors.

MR. MOSKOWITZ: Could you briefly describe the instrumentation used in making your mappings?

MR. IUFER: Yes. Basically, we have a coil facility that attentuates Earth's field to a point where the induced magnetizations because of the external field can be ignored. We then expose the specimen after a previous deperming check to the standardized field. We then mount the instrument in a gimbal and rotate it in front of a flux gate sensor, which has a threshhold of 0.1 gamma and is oriented so its axis points toward the center of gravity of the specimen. The specimen is oriented in front of this magnetometer so that the axis of the magnetization points towards the magnetometer.

At one point in the rotation, and away from it (180 deg later) — perhaps I could illustrate this by saying that we rotate the sample in azimuth while its dipole, resulting from the exposure is horizontal — the radial field is measured and the characteristic of the trace is a sinusoid. By doing this, we are able to examine the recording at the start and stop of the scan to determine whether or not the ambient field has shifted. We take measurements to a resolution of 0.1 gamma, which is about 1% of the diurnal variation. It is necessary for us to determine whether or not the background fields have changed during the course of the measurement. By using this scanning technique, it is possible to evaluate field errors from external causes.

MR. MOSKOWITZ: I am also interested in the nature of the instrumentation. Is this a purchased magnetometer or something of your own construction?

MR. IUFER: Yes. We use an off-the-shelf magnetometer.

MR. BARNES: Barnes, Hughes Aircraft Company. Do you use an RF switch that one speaker referred to as a circular switch?

MR. IUFER: The answer is no. Are you talking about coaxial switching?

MR. BARNES: Yes, some kind of RF switching.

MR. IUFER: Yes, we use coaxial switches.

MR. BARNES: Some switches have massive, great magnets in them, and I don't know much about RF work.

CHAIRMAN GAUGLER: You mean circulators?

MR. BARNES: Yes. Apparently, you don't use one.

MRS. EBERHART: We do use five coaxial switches. They are not the circulator type. These were thought about quite a bit when we first started our program, but the field levels were much too high. We were able to work out a coaxial switch that does not use a permanent magnet but does use solenoid activation. This is what we used on Pioneer for switching. We tried to avoid as many magnets as we could.

MAGNETIC TESTING OF SPACECRAFT

C. L. Parsons
Goddard Space Flight Center
Greenbelt, Maryland

My message is brief because my business is testing and I believe in it firmly; in a nutshell my message is test everything as often as you can, and do something about it afterwards – after you have the results. One of the difficulties in starting late on a program like this is that a lot of your thunder is stolen. I have an introductory sentence here that is almost identical to two previous ones. I'll read it anyhow just to see if it compares any better.

To measure the magnetic field in space and to study the magnetosphere, one must place in orbit a spacecraft equipped with a sensitive magnetometer. This requires that the magnetic disturbance, because of the spacecraft, be kept to a level well below that of the field being measured.

I would like to bring in at this point one thing that hasn't received much attention, and that is the attitude control. We've all spoken about the problem of flying the sensitive magnetometer and trying to reduce the disturbance, but there are many who are concerned with the attitude control, the spin-stabilized payload, and its torque disturbance because of its magnetic moment, coupling with the Earth's field; and a corollary of that: of using this torque to produce attitude control on command.

So, we are approached at Goddard frequently to investigate this sort of magnetic device and do this sort of testing.

In Table 1, I have outlined very briefly these three basic problems. First, of building the nonmagnetic satellite detector.

Table 1. Magnetic testing

Ultimate purpose: to measure magnetic field in space.
Probe: flight magnetometer
Vehicle: spacecraft
Problem no. 1: magnetic disturbance of spacecraft obscures magnetometer measurement of field in space.
Problem no. 2: magnetic moment of spacecraft couples with the Earth's magnetic field causing attitude change.
Problem no. 3: attitude and spin-rate control.

In working with the measurement of magnetic fields because of any of these devices, there are various types of magnetization that one must watch for; Table 2 shows these. You have already been informed of some of these. The permanent magnetization, which I like to break into three types: the initial, the maximum, and the final. We measure initial. Mr. Iufer mentioned that he didn't take the initial perm measurement, but we take it on receipt of the object in question.

Table 2. Types of magnetization

Permanent: initial, maximum, and final.
Induced: proportional to applied field, permeability, volume, demagnetization factor.
Stray: proportional to number of turns, current, area enclosed, core material permeability.
Eddy current: proportional to spin rate, applied field, conductivity, area.

We then expose to the 25 gauss, which has previously been mentioned – this value, by the way, is selected to represent that magnetic influence that the device may see as it passes through its environmental test program including the ever-present shaker tables. Then, finally, we deperm the sample and measure the remnant perm. We also try to determine the induced moment, and with the induced moment we work with the coil system. We work our perm measurement in a zero ambient field, and then later we apply an artificial vector field at a known direction and magnitude; from this, note the induction or the induced magnetization that occurs.

This, of course, is proportional to the magnitude of the applied field, the permeability of the material, the volume and the demagnetization factor, or shape factor of the particular item being tested.

We also investigate the stray field, both from packages and from the spacecraft later in the complete system. The stray, of course, is proportioned to the number of turns through which the current is passing and the area enclosed by them; the core material permeability, if there is any, will all influence the magnitude of the stray.

Finally, we attempt to look into the eddy current problem. I haven't heard much mentioned of the eddy current problem today, and I am not in a position to talk too much about it either, because it is a very difficult thing to investigate. But one

wonders what happens when you spin a payload in an ambient magnetic field. Eddy currents will be generated and the conducting members, in turn, will generate a magnetic field of their own, which will be in opposition, and we would like to know what these are in connection with spin decay rates, for example. We hope soon to be able to look into those working with our new facility, which we will have soon.

Figure 1 is for the benefit of the beginners, if there are any (and I am beginning to think that there aren't) in magnetism who might not comprehend the significance and the difference between a perm field and an induced field.

I have tried to show here that the large square item labeled "induced" is a piece of an iron box, and the Earth's field is always from left to right. The permanent field, however, is fixed in the framework of the satellite, which is represented by the circle.

Now, as you rotate this gradually about a spin axis, for example, obviously the perm field rotates with the package. The induced field, however, is tied to the direction of the ambient or induction field. And so its influence on a device located at the midpoint, such as a detector, would vary as you rotate, and, of course, this is the very feature that makes it possible to separate the perm from the induced, when you are working in the Earth's ambient field, if you must.

Table 3 is an indication of test requirements.

Table 3. Test requirements

Controlled environment: magnetically quiet area, coil system to null Earth's magnetic field.
Motion fixtures: gimbal for rotation, carriage for translation.
Exposure fields: perm and deperm coils, rotating fields.

The control environment — I think most of you will agree we need to have a magnetically quiet environment in which to work. There is, perhaps, some disagreement as to whether we need that coil system that is listed there to null the Earth's field. I, personally, feel that we do. I find it to be very useful in our work; I think it speeds up our results, and hopefully improves the validity of them in some cases. Some of the difficulties encountered, however (in really getting a clean zero background), are daily variation of geomagnetic field and the uniformity of fields generated

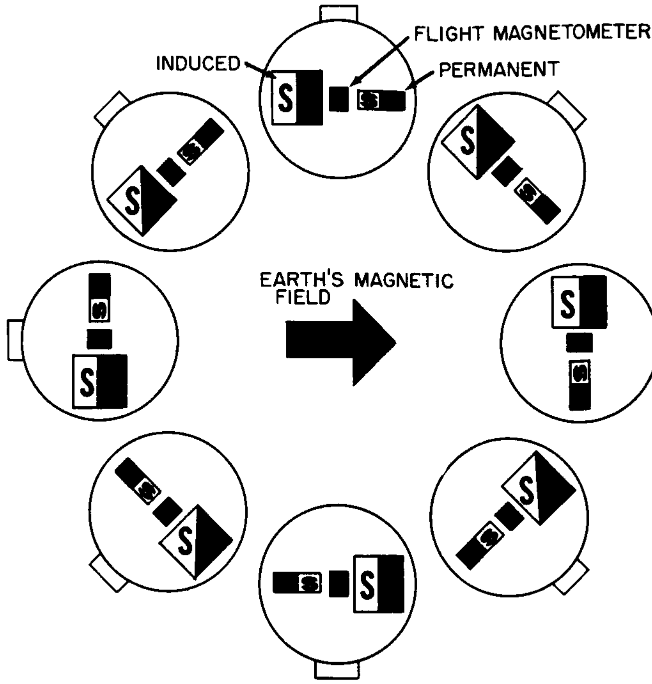


Fig. 1. Permanent and induced magnetization aboard spinning spacecraft

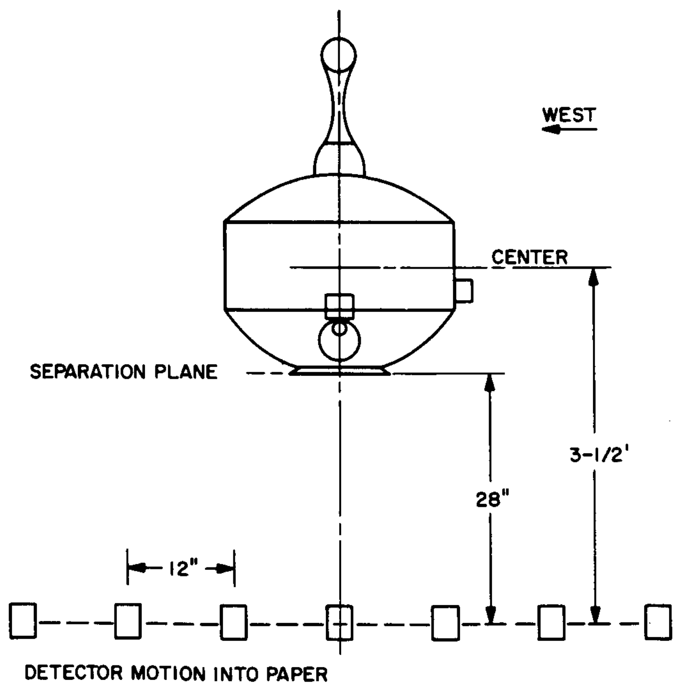


Fig. 2. Arrangement for tests

by the particular coil system. It should be adequate, of course, to cover the volume of the device being tested.

I use motion fixtures of various kinds – beginning originally with a translation system, a carriage with which to move either the detector or the payload; later with gimbal fixtures on one axis and finally on two axes, as Mr. Iufer described a moment ago. A double axis gimbal system, seems to me, gives the quickest and most sure-fire method of determining field.

There is another area associated with this motion, which generally I refer to under the title "torque meter," the possibility of determining the torque that is generated by the moment at the payload acting with the ambient field. If you can measure this torque mechanically and accurately enough from this, one could then determine the moment; and this is an area that I will talk a little more about later.

The exposure fields, of course, perming and deperming coils are required with which to generate the necessary 25- or 50-gauss fields.

Methods of measurement - In Table 4 I have proposed here four possibilities. Perhaps there are still others. Obviously, one can hold the spacecraft stationary, move a detector past it in various ways, to get some idea of its field. One can instead translate the spacecraft on a carriage and observe the field with a stationary detector. One could rotate the spacecraft on one or more axes and note the field with stationary detectors. Finally, one can attempt to read or measure the magnetic torque (the mechanical torque generated by the magnetic moment). All of these we have used at one time or another. I will give some data from some of these in a moment.

Table 4. Methods of magnetic measurement

Spacecraft stationary, detector translating
Spacecraft translating, detector stationary
Spacecraft rotating, detector stationary
Spacecraft rotating, torque measured

Figure 2 is an arrangement that we used in the investigation of the international satellite, UK-I spacecraft some time ago.

We were, in this case, holding a stationary craft with detectors passing below. In general, this data was taken too close to the source to be really a moment measurement. Generally, the rule of thumb that I like to use is that one should be at least three times the maximum of the magnetic envelope away from the object being

measured and if it is an oddball object, with moments that are located near the rim or perimeter, then six times would be preferred. One can verify, to some extent, by noting the magnitude of polarity peaks and also by taking data at more than one distance and noting the rate of falloff to see that it is, in fact, following the inverse cube as it should, if you are far enough out to treat it as a simple dipole.

Figure 3 is an indication of the type of data that we got from this setup: a survey of the field over the plane below the payload, a contour developed from it. This is useful for some interpretation, but it is really more cumbersome than necessary in many cases.

Figure 4 is an artist's concept of our facility in which these tests were first done. This shows the magnetic ship models laboratory as it was arranged back in the Fifties, beginning in 1949 up through about 1961 or 1962. This, of course, is showing a ship model located there. The black blob in the center was the ship model. The large coils surround the entire building — the coils, for example, run across the basement, rise vertically through the first floor clear up through the ceiling, through the attic, and across and down. These are square loops — in the case of the X-coil circuit, there are six of these loops. The basic dimension here is 40 by 30 by 30 ft.

It was intended, of course, to produce a relatively uniform field over quite a large volume for purposes of magnetic ship model testing. But it lent itself fairly well to the investigation of the first few spacecraft that were brought over, and the carriage below here, is one of the two that are used for translation-type data.

Table 5 indicates what one would like to do. This bears a little more closely, perhaps, to the topic of the earlier papers. Obviously, the best way to make it nonmagnetic is don't put any on there in the first place. So, get rid of the ferrous material if you can. If you have a DC circuit, rewire it, twist the pairs, or install a compensator loop that will parallel the path of the other.

Table 5. Analysis and reduction

Remove ferrous materials
Rewire DC circuits
Install magnetic shields
Compensate with magnets
Deperm
Degauss with coils

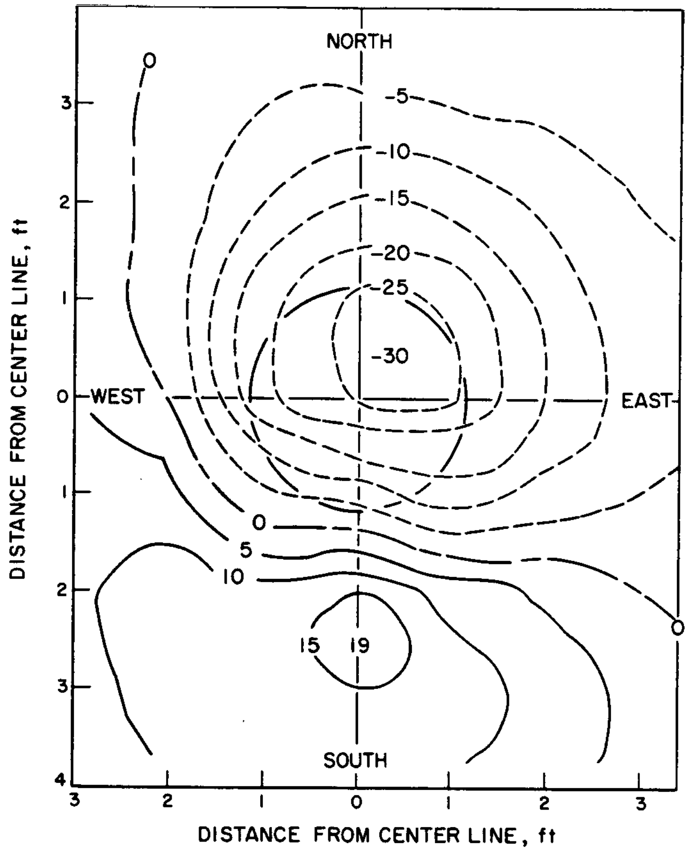
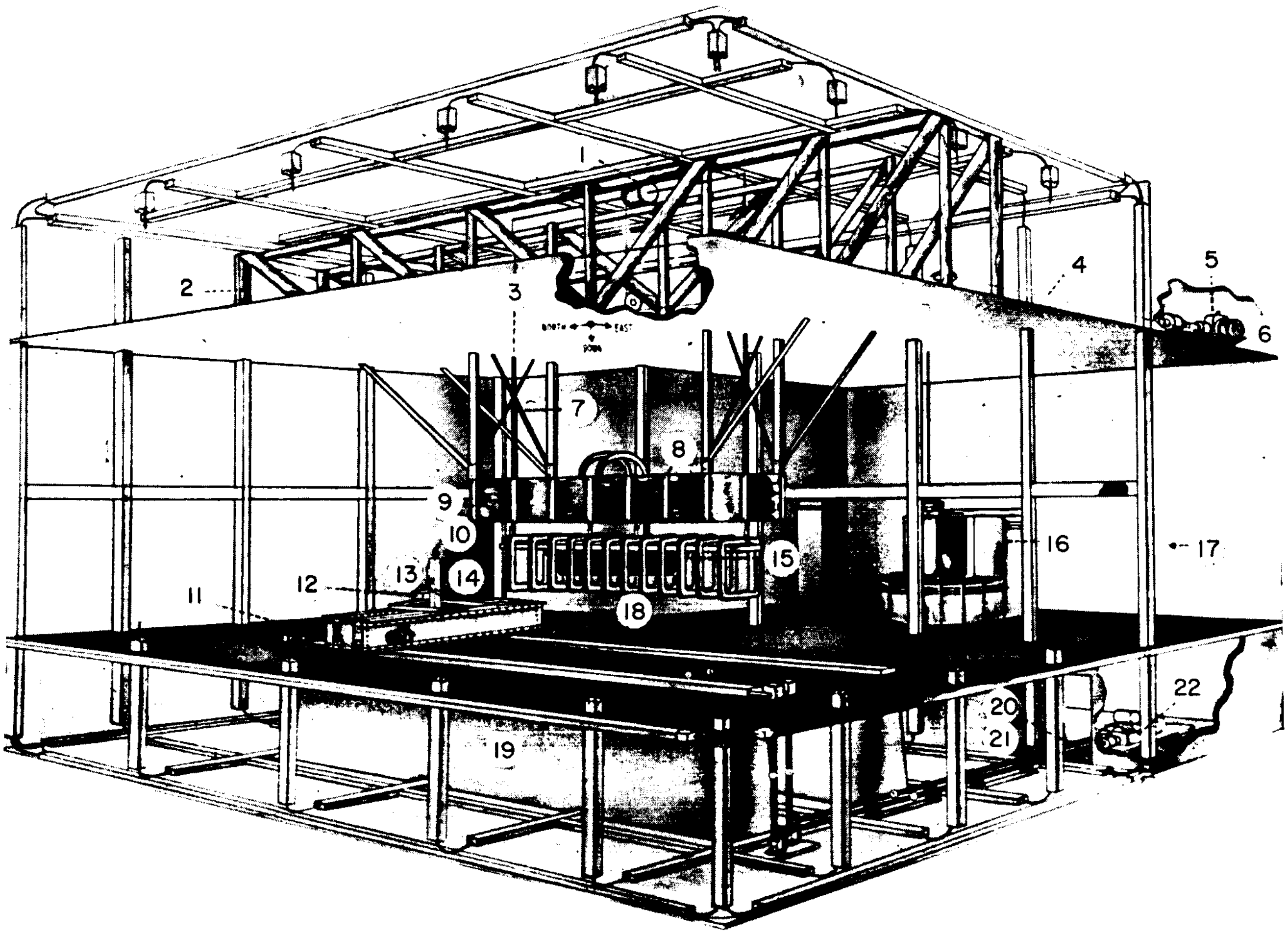


Fig. 3. Magnetic field contours, units are in gamma



(1) CABLE DRUM
(2) MAIN SUPPORT TRUSS
(3) MODEL HOIST SYSTEM
(4) PHOSPHOR BRONZE CABLE
(5) MODEL HOIST DRIVE SYSTEM
(6) CABLE DRUM
(7) MODEL MOUNT
(8) AUTOMATIC HOIST LATCH
(9) Z DEMAGNETIZATION COIL
(10) MAGNETIC FIELD DETECTOR
(11) SILICON BRONZE ROLLER CHAIN
(12) SYNCHRO DRIVE FOR VERTICAL MOTION (ON TRANS CARR)
(13) LONGITUDINAL AND TRANSVERSE CARRIAGES
(14) TRANSLATOR TOWER
(15) SHIP MODEL
(16) MASTER CONTROL CONSOLE
(17) CONTROL ROOM
(18) X DEMAGNETIZATION COIL
(19) CONCRETE PIERS
(20) NO.2 DRIVE FOR TRANSLATOR
(21) NO.1 DRIVE FOR TRANSLATOR
(22) TRANSLATOR DRIVE GEAR BOX

Fig. 4. Magnetic ship models laboratory constructed entirely of nonferrous materials at the Naval Ordnance Laboratory (NOL)

Magnetic shields we have already heard something about; we have used them with some degree of success. Ninety or 95% shielding on occasion is not unusual. Though there is always a question about whether you are able to maintain stability with this shield; and also whether you eventually build a shield so large as to produce an induced moment so large that you're no longer gaining anything by adding the soft iron material.

Another method, which I don't have on the outline, is the orientation of components, placing back to back, as I call it. You have identical latching relays, or identical anything that do have permanent magnets in them; by orienting every other one you can self-compensate the total assembly to a fair degree. We have gotten up to 80% this way on occasion, sometimes better. I include in my outline the degaussing with coils. However, we haven't yet done that; this was a proposed method that may eventually be used.

Figure 5 is the first satellite test that I was engaged in. This was in January of 1960. The Applied Physics Laboratory of Dr. Bob Fischell came over and brought the Transit satellite for initial investigation. That satellite, again, had a device for attitude control; namely, a magnetic moment generated by 1 by 4 in., I believe it was an Alnico Magnet put on the spin axis, intended to cause the payload to tumble as it passed near the Earth's north or south pole (which it did, I understand, very successfully). It also had a magnetic despin system. I take no credit for these systems; don't misunderstand me. I am just a tester. All I do is investigate the box or package or whatever, and perhaps offer some advice as to what might be done on occasion to reduce the field. But this is an Applied Physics Laboratory spacecraft, and I want to make that point very clear.

The next payload we worked on was the Explorer XII in Fig. 6. This was the S-3. In February of 1961 we measured its fields and I will give some of these fields later in a summary at the end of my presentation.

Figure 7 is, again, a satellite. Basically the University of Iowa, I believe, was responsible for a lot of this. Again, APL (Applied Physics Laboratory, John Hopkins University was assisting, and we were serving as a test facility and as testing operators. Figure 8 is another satellite from APL, TRAAC.

Figure 9 is Explorer XVIII, the IMP-1 — this was rigged in one of the fixtures used in the testing and checkout of the onboard flight magnetometer. Again set up in the old facility at the Naval Ordnance Laboratory.

Figure 10 is this same style, the IMP spacecraft, but this time it is set up in our new facility at the Goddard Space Flight Center. This coil system has been

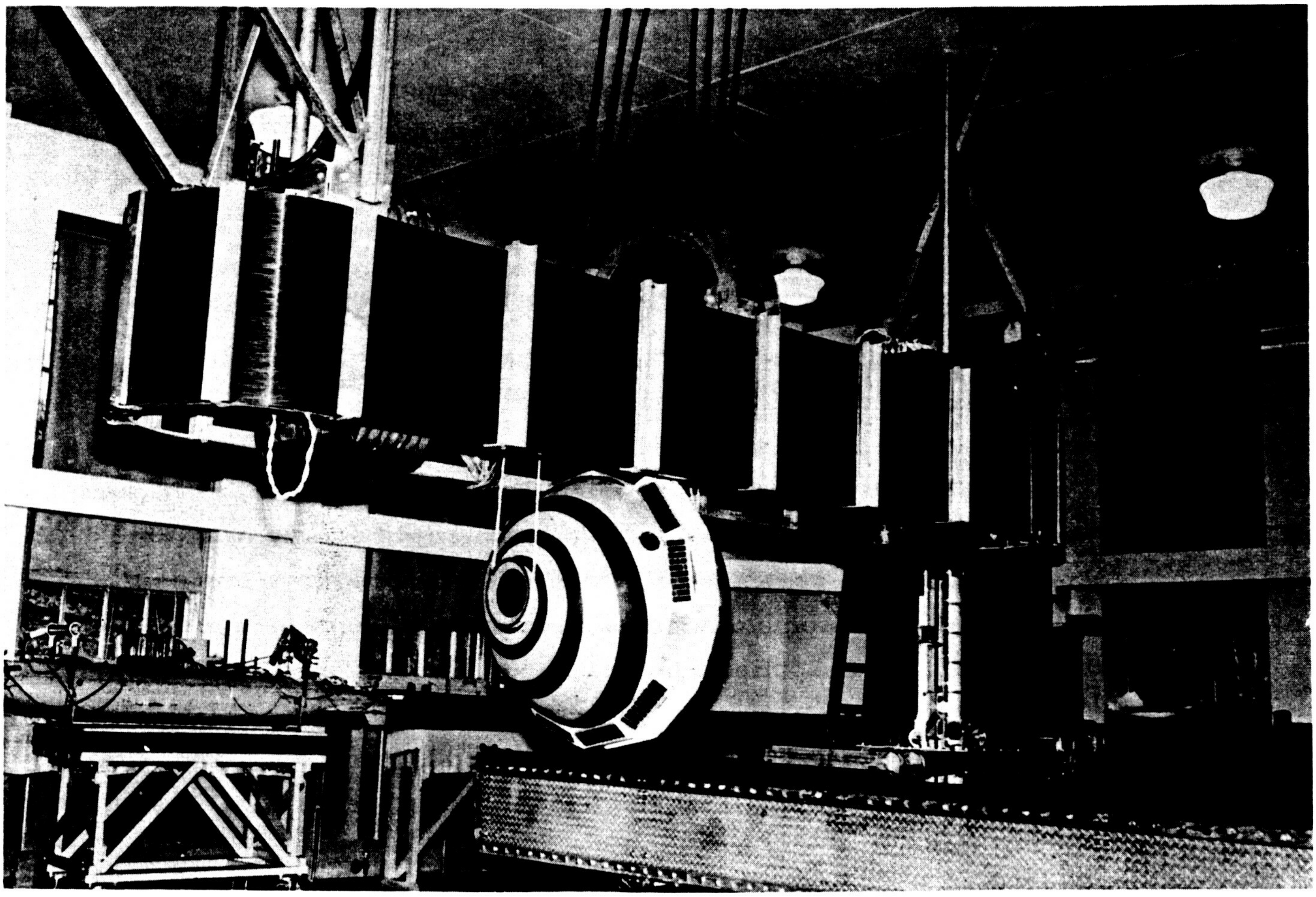


Fig. 5. Transit satellite at NOL mapping facility

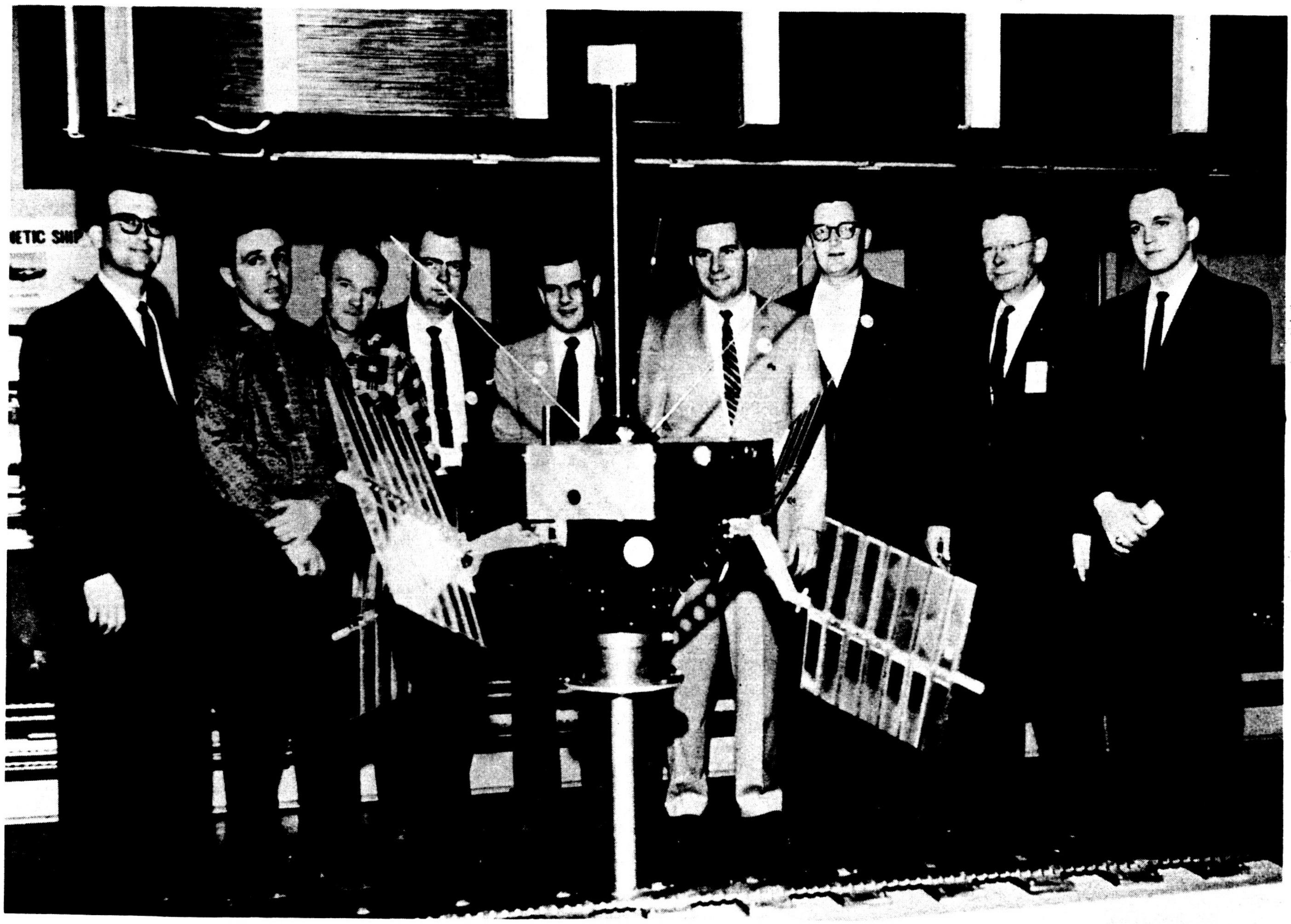


Fig. 6. Explorer XII

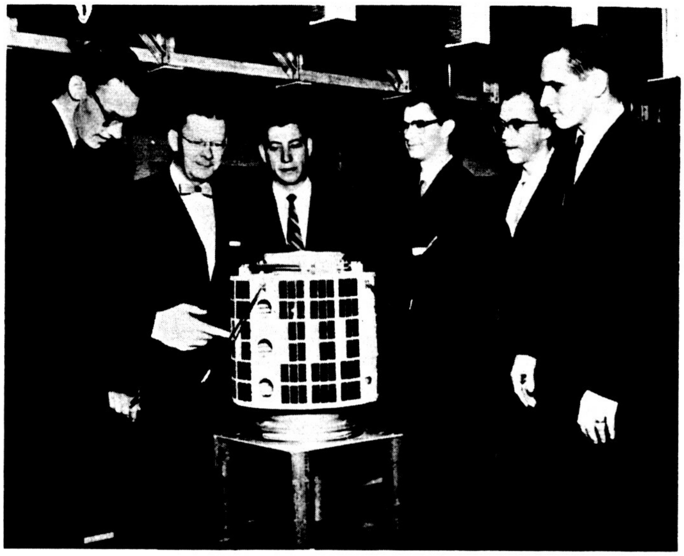


Fig. 7. Injun satellite

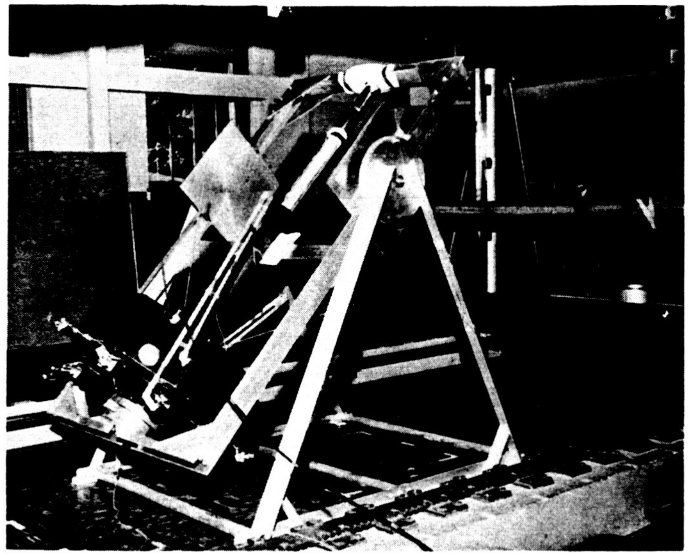


Fig. 9. Explorer XVIII (IMP-I) at the Naval Ordnance Laboratory

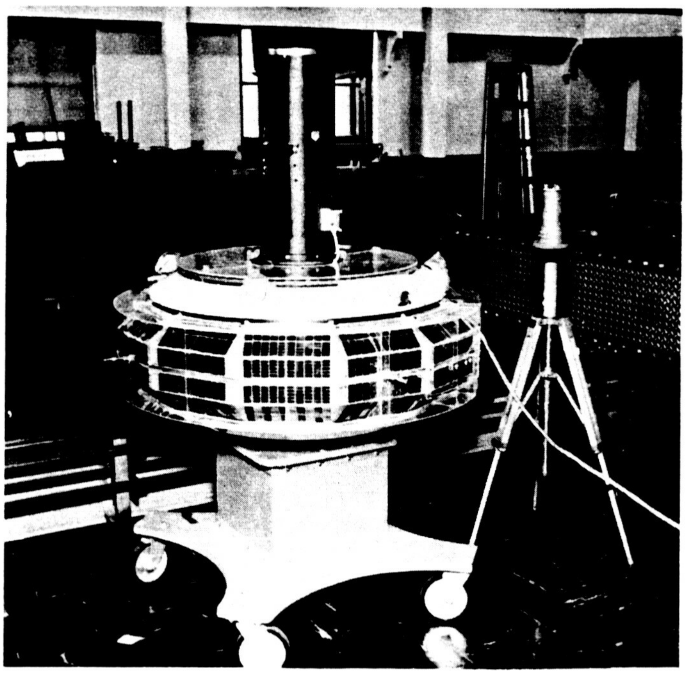


Fig. 8. TRAAC (transit research and attitude control) satellite

Fig. 10. Explorer XVIII (IMP-I) at the Goddard Space Flight Center

erected for some time. The control system for it is not quite completed. You will hear more about these Goddard facilities tomorrow, when Mr. Brown gives his paper. This is just to show the setup that was used for a checkout of a flight magnetometer. It is the first test attempted in our new facility.

Figure 11 is a checkout of the international satellite, UK-II. Now, this time we are using the two axis rotation. As you can probably see, the table below is a king-size "Lazy Susan" of nonmagnetic material. On it is a frame supporting a horizontal axis, permitting rotation along this axis so that we could rotate by one of our detectors located in back. We found, by the way, one of the best nonmagnetic orientation devices you can get. You don't just fool with these remote control devices if you can help it.

Figure 12 is the Canadian-U. S. satellite, Alouette B. This is one of the satellites that raises the problem that I will just briefly mention.

This payload was too heavy to be turned on its other axis without taking time to build an elaborate fixture. So we were limited to either rotation on a single axis — the vertical-spin axis — or the translation of the payload in and out. Now, I maintain that if you attempt to do this kind of a test in the Earth's ambient field, you will find it difficult — well, in fact impossible — to separate the induced moment in the vertical axis from the perm moment in the vertical axis. Unless you can tumble the thing end over end; you need some means of compensating the Earth's ambient field, at least a vertical axis coil system.

Going on, then, we come to proposed tests. Figure 13 is the dummy of the OGO-B. It has been placed on a newly-designed and built track, turntable, and dolly, which we put together for this purpose.

Figure 14 is the same fixture with the dummy viewed from above. It just permits us to roll on the rings. We can rotate on the vertical axis and we can translate rapidly and easily from one end of the track to the other. This business of rapid motion, by the way, is aimed at minimizing the influence of all the various drifts and disturbances that are occurring to your magnetic field. Those of you who have actually worked with magnetometry, especially at the 0.1 gamma level that was mentioned a while ago, know that things just don't stay stable to 0.1 gamma for any longer than it takes to take a deep breath. It is very much to your advantage if you can move the package in and out hurriedly and do the rotation and get it over with before things begin to drift out of control.

Again, we hope to overcome some of this in our new facility, because we will have geomagnetic tracking and feedback correction.

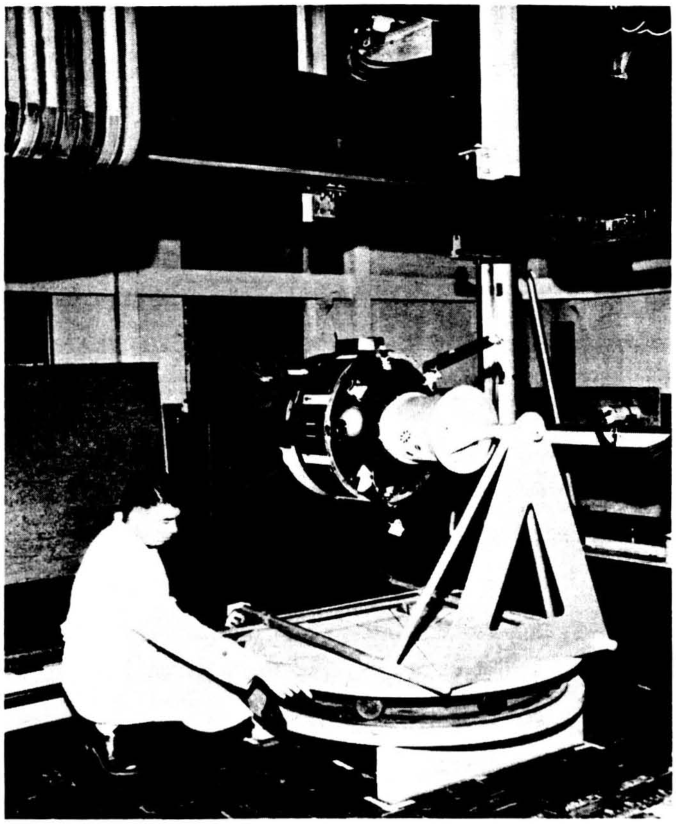


Fig. 11. International satellite UK-II

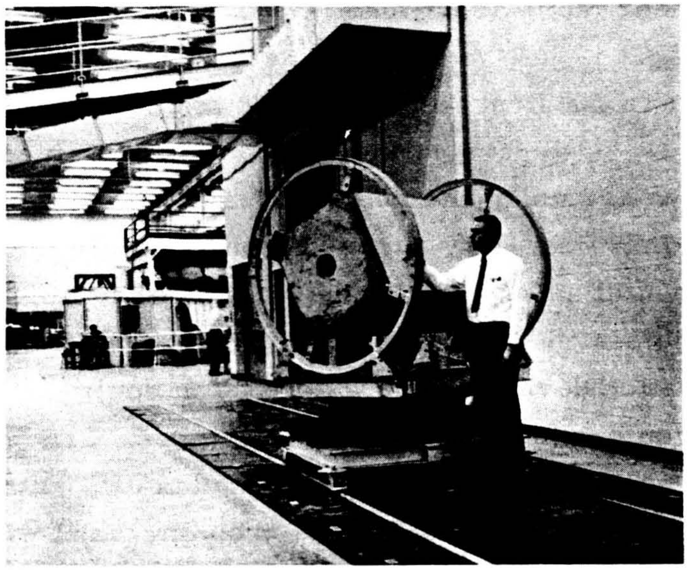


Fig. 13. OGO-B dummy

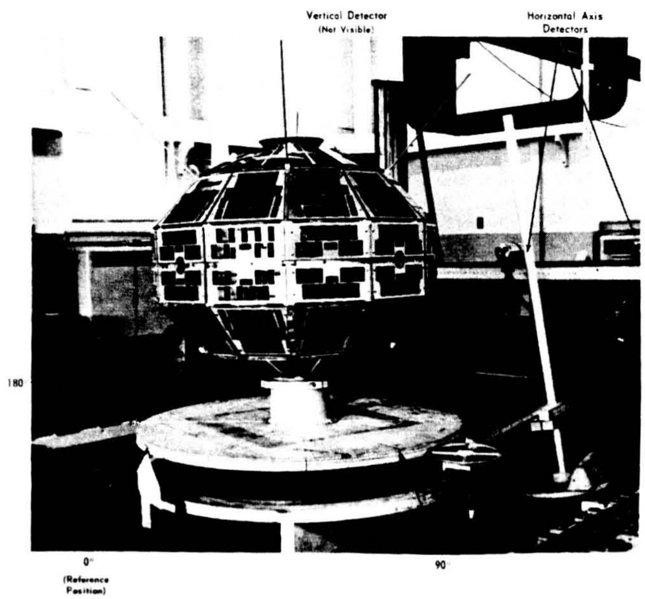


Fig. 12. Canadian - U. S. satellite
Alouette-B

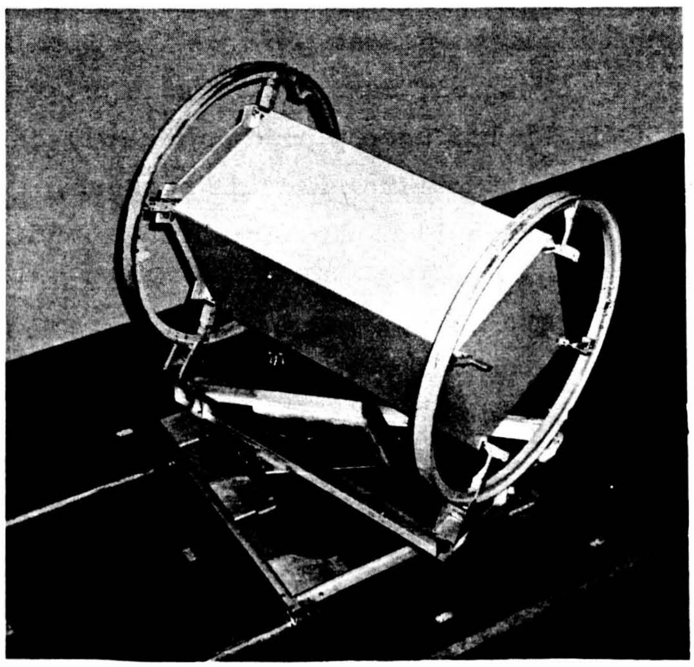


Fig. 14. Top view of OGO-B dummy

Figure 15 is an attempt to show you a proposed setup on the OGO-B, Phase 2 (as we call it) magnetic interference test. This is set up in a newly-constructed building of nonferrous material at our new facility at Goddard. It is a little hard to make out, perhaps, but you see the basketball in the middle; it is the flight magnetometer —the rubidium located at the end of the EP-6 boom, and it is in the center of a small 12-ft aluminum frame coil system, which will be used to bring it to something reasonably close to zero field and with a uniformity adequate to permit it to operate.

Meanwhile, further down the end of the boom is the main body, the roll rings, and on the other end would be the EP-5 boom; the solar panels will be in the vestibules to either side. We have a very tight fit, I am afraid. Figure 16 shows this arrangement from the other end. Our building was short about 9 ft, so we put a little enclosure outside the door to protect the EP-5 boom.

Figure 17 again is another view from near the main body, looking down at the EP-6 boom to the flight sensor, in which you get a little better idea of the shape of this 12-ft Braunbek-type coil system that has been constructed for this purpose. We have already tried this coil out. It has just been completed. We did put a flight magnetometer into it and did find, in fact, that we could operate it. There was some question in everyone's mind about this, because the rather extreme uniformity of field required to make the flight probe behave itself.

Now, all this has brought me down to my summary table, and I just want to say a few things first.

One thing, I have tried to gather together all the data available that describes the magnetic status of many different satellites. To make the data easily comparable, I have converted it all to so-called magnetic moments in cgs units. I do offer this caution however, some of the data was taken actually for the purpose of determining the disturbance that the flight detector would see, and so it was taken a little too close to be a really valid moment measure. So, some of these numbers, you will be able to tell which, are a little on the weak side — maybe 20% inaccurate. But it is the only way that I could pack all this data together.

Some of these tests were done by other than my own men or myself, and I have tried to indicate that. There was nothing intentional about leaving out any particular flight program it was only because I just didn't have the data available. This is shown in Table 6.

There are 26 different tests represented. I have tried to show the data on which the test was run, and whether it was a flight unit or not; what we call either a final, an initial, or a post-environmental test. Then the moment believed to be due

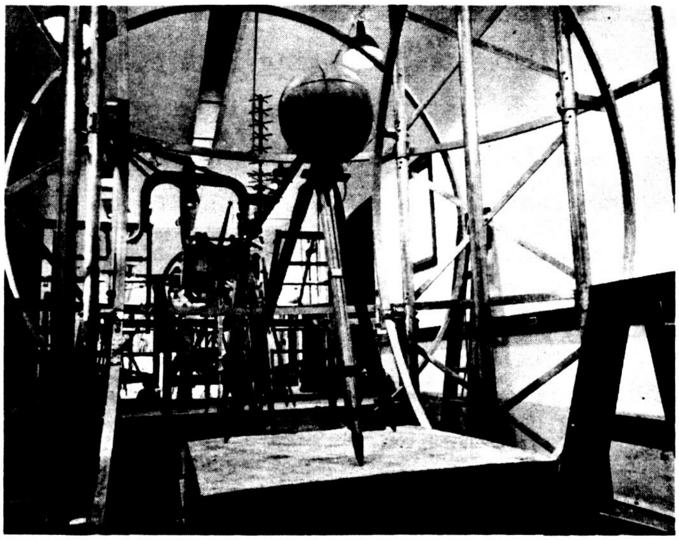


Fig. 15. Structural model of OGO-B viewed from the EP-6 boom

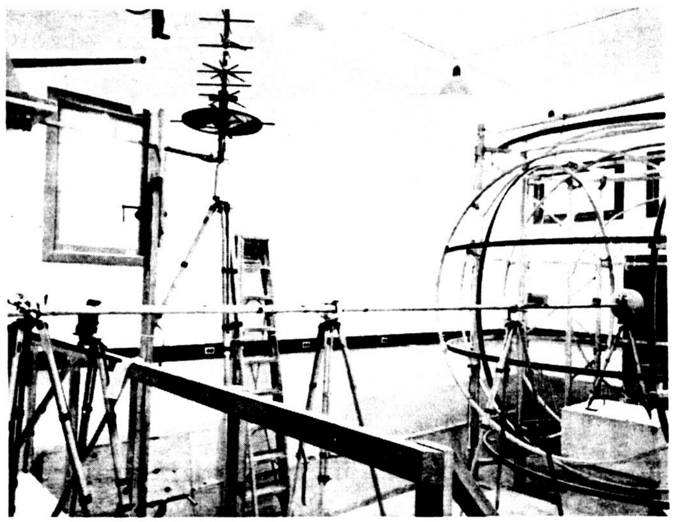


Fig. 17. Structural model of OGO-B with EP-6 boom in 12-ft coil system

Fig. 16. Structural model of OGO-B viewed from EP-5 boom

Table 6. Spacecraft magnetic moment summary (magnitudes in cgs units)

Spacecraft			Date	Test	Initial perm	Induced H 0.38 gauss	Stray	Post 25 gauss perm	Post deperm
Transit		Flt	1/21/60	Final	31000		8		
Explorer XII	S-3	Flt	2/16/61	I & F	92	369	19		
*Tiros	I	Flt	4/1/61	Final	896				
Injun		Flt	5/9/61	Final	5942				
Explorer XIV	S-3a	Flt	9/4/62	Final			126		49
Explorer XV SERB	S-3b	Flt	10/12/62	Final			28		12
EPE-D	S-3c	Spr	10/19/62	Final			32		16
UK-I	S-51	Pro	12/4/62	Final	67	117	134		
Explorer XVIII	IMP-I	Flt	8/8/63	Init	88				26
Explorer XVIII	IMP-I	Flt	8/12/63	Post-E				377	31
UK-II	S-52	Pro	9/4/63	Final	734	398	76		568
UK-II	S-52	Pro	9/4/63	Final			Compensated		382
UK-II	S-52	Flt	9/11/63	Final	612	581	31		612
UK-II	S-52	Flt	9/11/63	Final			Compensated		184
Explorer XVII	S-6	Pro	10/4/63	Final	29983				
Explorer XVIII	IMP-I	Flt	10/23/63	Final	213				31
Explorer XVIII	IMP-I	Spr	10/23/63	Final	34				24
*GGSE	NRL	Flt	10/31/63	Final	254	181	9		
Explorer XXVI	EPE-D	Flt	4/14/64	Init	83	52	26		56
*OGO-A	S-49	Flt	4/15/64	I & F	5872				2349
Explorer XXI	IMP-B	Flt	4/28/64	Init	61				
Explorer XXVI	EPE-D	Flt	6/3/64	Post-E	49				40
Explorer	IMP-C	Flt	7/30/64	Final	38		113	630	26
Explorer XXI	IMP-B	Flt	8/14/64	Final	23			490	19
Alouette-B	S-27	Pro	8/17/64	Init	1160	670	778	5950	1160
Alouette-B	S-27	Pro	8/24/64	Final	1232				2558
Explorer XXVI	EPE-D	Flt	10/8/64	Final	67				61
Explorer XXVI	EPE-D	Spr	10/9/64	Init	57	16			47
*OAO	I	Pro	11/7/64	Init	9911				

*Test performed by others

to the initial perm, and induced moment if we have the figure for it; a moment that is the equivalent of the stray field seen, then a post-25 gauss perm exposure, and finally a post-deperm field.

Again, that first section of the table was done back in the days when we were working with about 20 gauss of 60-cycle deperm field. Later we went up to higher fields, the deperm results were somewhat better as a result. Many times people come to me and say, "How much magnetic moment does a typical satellite have?"

Well, as you all probably know by now, there is no such thing as a typical magnetic moment. They are all different, and some widely different; so I have accumulated this data over the past 5 yr and I offer it here just as an insight.

I understand some of you people are somewhat new in the area, and perhaps would gain something by getting just a little feel for what these have in the way of magnetics.

Some of these have been compensated, as I mentioned, the UK-II shows the field before compensation and after compensation. We had hoped that this would be a better result. This was the prototype unit — the flight unit was a little better; we had a 612 post-deperm field, which compensated to 184 after the addition of the compensating magnets. You notice some of these have quite staggering magnetic moments. This is because they contain quite staggering magnets.

The S-6 has horizon scanners, top and bottom, each of which contains a nice, big, monstrous magnet; and then it has a so-called "redhead gauge." If you ever have a satellite program come to you that says something about "redhead gauges," turn and run the other way if you are a flight experimenter, because they are horrible sources of magnetic fields.

On the other hand, some of these fields have come down quite low. Here is a 24 over here, for example, on the IMP-I that was a post-deperm result. We have other data on the various IMPs — some flights, some flight spares.

I might just say the last one, the OAO, we had very little to do with that test ourselves, it was run by others. We did offer some advice, and we had a man there to observe. This was one of the cases where they would not permit this satellite — or observatory, I should call it — to be tipped over. It could only be rotated on a vertical-spin axis. They had no facility or coil system in which to work. They did remarkably well considering these limitations. But I still say that the influence of the vertical induced moment is unaccounted for, because there is no way to get it if you can't tip it over.

I find it beneficial, especially when introducing the subject to a new person, to be able to point out the various levels of moment that you may run into. As far as the remnant field existing at the flight detectors on some of these programs — I have that data also, if people are interested later on — the EPEDs or some of the early ones, originally called the S-3 or the SERBs. These were pretty clean flights. They had initial perms like 15, 12, 11, and 16 gamma; after deperming, these dropped down to 3, 6, 2, etc. These were the predecessors of the IMP program that, as you heard a while ago, has finally dropped down to a post-deperm level of less than 1 gamma in all cases, and in a few cases less than 0.5 gamma.

I want to repeat my opening remarks: test everything as often as you can, and do something about it after you see the data.

OPEN DISCUSSION

MR. NORRIS: You showed the OGO structure being moved on a track, and you mentioned that you were measuring the field by moving the bus along the track; is that in a zero field?

MR. PARSONS: The OGO Phase 1 test, which will be the measurement of the moment, will be done in a near zero field inside a 27-ft cubic coil system. The measurement is made while the payload is at the center of this coil and being rotated on its vertical or on its roll axes. Immediately afterwards, it is rolled down the track and out of the facility. This is only a means of getting it rapidly away from the test instruments, which will be inside the building.

MR. NORRIS: So it does start out, then, in a zero field?

MR. PARSONS: It will be in a zero field within something like 1/2% of uniformity while the measurement is made.

MR. NORRIS: Do you have any estimates of what the eddy current induced fields do to you in that type of measurement, it looks like a lot of metal there.

MR. PARSONS: The motions involved here are very slow, and it is expected that the eddy current generated in any of this structure would be negligible, unreadable. By "slow," I mean about one revolution in 2 min, or something of this sort.

VOICE: Could you tell us how they measured the OAO field in this vertical satellite?

MR. PARSONS: Well, yes.

What they did was to build a track and a dolly similar to the one that I have here; and when they told me they couldn't tip it over, I advised them to put a detector in a hole below the center of rotation before moving the payload over, and also to have detectors in the horizontal plane at the midheight or at the height of the cg, which they did. They took data, then, on axis "in versus out" as I call it, that is, they set zero and move the thing in and read the field and move it out. This gives the vertical moment. However, it doesn't tell you how much is due to induced and how much is perm.

Then they rotate it and, by monitoring with the horizontal probe, they have got a pretty good idea of where the moment is in the other direction. Then they did some extensive manipulations that I was not in on, and came out of this with a fairly strong statement as to exactly the orientation and magnitude of the moment at the payload. There was no tumbling over, just rotation and removal. There is one thing to be said about induced moments, of course, when you go in orbit, the induced moment is going to vary with the magnitude of the field in the orbit; and in some cases this may be quite low. If you are in an elliptical orbit that throws you off in space, you don't have to think about induced moment at all. If you are in a circular orbit, chances are that the field will be between 0.2 and 0.4 gauss; and at that level, if you have soft iron aboard, you are going to see some effect from it. An example of that is the ferrite antenna loops on the UK-II satellite, for example; it had quite a pronounced induced moment.

You can't throw away induced moments entirely. In my opinion, you have got to watch for them. In some cases, you won't be disturbed over them.

MR. GORDON: In your table, for the Alouette, you showed that the post-deperm moment was about twice as big as the initial moment. Is this good?

MR. PARSONS: This is one of those embarrassing moments that one dreads. We did deperm the thing, and it did appear to grow larger as a result. Now, present interpretation of this is, we knew that it contained a good many sizable magnets and we knew there was a good amount of ordinary "run of the mill" ferrous material in there.

At the time we received it, we were of the opinion that we had a normal perm, which was in opposition to that of the hard magnet perm. As we gave the treatment,

we knocked out easily the so-called normal perm or semiperm, or whatever term you want to apply to it; but, of course, we did not deperm the Alnico magnets and so, as a result, we ended up with a magnetic moment that was larger than what we started out with.

Now we say it is better this way from one point of view; because it is stable. We say we have got the hardest, cleanest piece of perm you could ever want. All you have to do is compensate it with a magnet and you are all set.

MR. MOSKOWITZ: Just so you'll know you are not alone; we observed a similar phenomena on the Tiros satellites where we have taken the batteries and depermed them and found out we have a larger moment as a result. In which case we added permanent magnet compensation to the whole vehicle. Also, if you are interested, I have a table of additional Tiros satellites — the moment that it had on ground, the moment prior to launch, and the in-flight measurement of moment, which I can let anybody have.

MR. PARSONS: One question about your deperm treatment. When you depermed did you have a zero ambient field in which to do the deperm treatment?

MR. MOSKOWITZ: Well, yes and no. We made measurements two ways. In a coil system, by rotating the spacecraft, in which case we had a similar coil, spherical coil, that we used to cancel the field. We also used a magneto static procedure, in which case we didn't cancel out the geomagnetic field. The results were very close.

MR. PARSONS: I don't mean in the measurement of it, but during the deperm treatment, when you applied this deperming field, was the ambient background a clean zero DC?

MR. MOSKOWITZ: No. During the deperming procedure we operated in a geomagnetic field.

MR. PARSONS: This, I would like to say, can lead to an increase in perm very readily, if the specimen in question had a field that was pretty low to begin with. Let me quote four numbers; we had occasion recently to do a deperm experiment on a rocket motor, one of these retrofire rockets that is going to be used on the IMP-D and IMP-E. We find that if you take this thing and expose it to a 25-gauss field, you will see 2300 gamma at 3 ft. This is a tremendous field; and to think that that is going to fly on an IMP breaks my heart.

But, if you take that and put it in Earth's ambient field and cycle it with an AC deperming field, you'll drop this level down to about 640 gamma at the same distance. If you put it in a zero field, zero DC field, and give it the same 60-cycle deperm treatment, it will come down to about 3 gamma. So, you see, if you were in a level of perm between zero and what we used to call in the degaussing business "equilibrium magnetization" and you shake it in Earth's ambient, it is going to perm right up to that equilibrium level.

However, if you were way up here in this horrible place to begin with, then you do gain something by deperming in Earth's ambient. But, if you want to really get it out, then you have to go to zero or there is one other possibility. That is what we call tumble deperm.

If you are sitting in Earth's ambient, and you can't get out of it, you put on this 60-cycle field and begin to tumble the package at random rapidly and randomly, and then diminish the 60-cycle field down. This way we got a result of 150 gamma, so I have four numbers.

In the worst case 2300; shaking in Earth's ambient, 640 or so; tumbling in Earth's ambient, 150; deperming in zero DC about 3.

MR. MOSKOWITZ: I just thought I would mention another curious effect we observed on the relay program. That is where we found, after rotating within this spherical coil system, that we needed a certain cancellation dipole moment. We measured and then proceeded to put it on a spacecraft, after which we found we were quite a bit off. Now in cgs system of units you are talking about a required cancellation of 5000 or 6000 units. We found that we were about 1000 off after cancellation. This was due to induced effects of the magnet on the spacecraft in other ferromagnetic material on the spacecraft. In an investigation of this effect, we didn't know what it was due to at first, I found that the effect can be either increased or decreased — it depends upon the relative location of materials.

CHAIRMAN GAUGLER: That is why I wonder if you have a permanent magnet and you get this sort of induced effect, whether deperming is really more stable or not. If you shake it around is it going to go back again? Of course, I guess it will go in the right direction.

MR. PARSONS: Well, this question of stability is a very critical and very important one, and I don't want to make too brash a statement about it. We are

taking data at every opportunity to try to establish stability of the perm treatment, deperm treatment, and any of these other things. Such systems, for example, as running a test on a package, taking it immediately up to the shaker tables (and we have one table we have investigated rather extensively), and we know places where we can put a sample that it would see nothing worse than 0.7 gauss for Earth's ambient, or machine-made noise either. We shake it there and bring it back and remeasure it; take it apart and measure each piece by itself, and so on and on through a series, and in general we find that things are pretty stable.

We expected to see instability in large quantity; but it hasn't been too awfully bad in the data we have thus far, and we are still working on that.

MR. TOSSMAN: I would like to direct this question to Mr. Moskowitz. You say you measured the magnetic moment of the satellite in flight, how was this done?

MR. MOSKOWITZ: This was done by attitude data that we received back from the satellite, and from the dynamics of the satellite we were able to predict the moment. It was worked out by Dr. Manguard, RCA, and by Dr. Bandine (I believe), of the Meteorological Service.

MR. LYNCH: You showed a double gimbal system for measuring the dipole moment or field in the figures. We feel that this system can be used to cancel out the induced magnetic effects — that is, the Earth's magnetic effects — if used properly. Do you find this true?

MR. PARSONS: As long as you have two axes of rotation, you can get a mathematical extraction, if you like, of the induced and the perm. Part of this has been done for many, many years in the degaussing program where the ship was too big to gimbal at all. The best you could do was change the heading, this was done and you got a readout of perm and induced, of course; the ship problem was very nicely assisted by the fact that the ship doesn't roll, or if it rolls, it only rolls once over so that you don't have this other variable to contend with.

Yes, it is possible to get the perm and induced separately by rotation on at least two axes. I don't use that method myself except in desperation. I prefer to take that induced effect completely away with the coil system first. But it is possible.

MR. IUFER: I would like to add one comment to what Mr. Parsons has said about the evaluating induced magnetizations by gimbaling. You have to make an

assumption in this rotation that the induced field you are measuring has suffered a phase reversal, and by proper summing in two positions, you are able to subtract out Earth's field. When you are dealing with distributed sources, as on an instrument shelf of a satellite, this may not be the case. You just rotate the satellite in front of a magnetometer; and if you could assume that the most significant sources were on the near edge of the shelf, the object would not only rotate in azimuth, but it would also translate practically the diameter of the shelf and on the far side the field contribution would be less by whatever attenuation law you might find. The technique has been developed for this, and this would be to have two magnetometers; so when the samples were measured at the two positions equally distant from the magnetometers, you have no translations during reversal or rotation.

N66-11282

MARINER MARS MAGNETIC MAPPING TESTS

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The title of this talk is "Mariner Mars Mapping Tests." However, my remarks are going to cover only the measurement of the permanent and induced fields associated with permeable materials, and only at the systems level; in other words, the spacecraft was virtually entirely assembled. Other speakers from JPL, I believe, will be talking about some of the tests made on individual components.

About August or September of 1963, a firm decision was made to magnetically map the Mariner Mars spacecraft, or the present Mariner IV spacecraft, by rotating it in the Earth's ambient field. Several items entered into this decision. First, a reasonable success had been had with the technique on Mariner II. The things that happened after launch are something that happened after launch and, in general, the measurements as made were considered to be satisfactory.

The Mariner Mars-type spacecraft was of similar characteristics, and a similar success would be expected. Secondly, and probably even more to the point, there was a very strong Project desire, if not an absolute Project decision, that the spacecraft was not to be taken off lab. A precedent had been established for this on previous spacecraft.

Thirdly, I understand that at the time these mappings were to be done, the local off-lab facilities were either not adequate or only marginally adequate. The spacecraft, in its final configuration, was quite large and, as a matter of fact, we didn't map it in this final configuration. I must say that I was not associated with this project or with the history that went into these previous comments to this decision, so any other comments will have to be directed to some of the people that were more closely associated with it. But, the decision was made to do this mapping in the Earth's field by rotating the spacecraft about axes that pass through the location of the flight magnetometer. We will get to the exact description here later.

My task was that of cognizant scientist for the procurement of a mapping fixture and setting up the mapping procedure. As a matter of fact, I was only to participate in some of the initial mapping operations, and was taken off and moved to another area at that time.

Well, with this introduction, I will now continue with the main presentation, and it will consist of four items.

First of all, a mathematical description of the test method of what measurements were made and how the data was reduced in a general way will be given. You will see it depends entirely on a linear theory, and we will discuss some of the aspects of this.

As a second item, I am going to attempt a few semiquantitative remarks about this business of lack of linearity in the real magnetic world. This may open up a lot more discussion, and we will just see what happens. I am going to put it out on the table, and let it set there.

Thirdly, we will talk about the test fixture, and give you some design details and some pictures showing what it looked like. One of the former speakers referred to a king-sized "lazy Susan." He should see what a real king-sized "lazy Susan" is, and he shall, as we show some of these pictures.

Lastly, I will present some of the slides showing some of the reduced data, so you can get an idea of what the results were on one of the Mariner Mars-type spacecraft. This was supplied to me just recently by Dr. Smith, and I am not sure whether it is the one that is currently on the way to Mars, or whether it was just typical of one of the spacecraft that was mapped.

Well, now, the first couple of items are going to involve a bit of mathematics. It may be a treat for some of you, and perhaps a treatment for others. I am going to try to go through it quite rapidly; unfortunately, it is really necessary to understand at least something of the method to appreciate why the fixture became the size and shape that it did.

The general method is described as follows. A suitable test area is selected (Fig. 1). This means an area that is larger than two or three times the size of the spacecraft and in which the inducing Earth's magnetic field, or the results of the Earth's field, is quite uniform. We will talk about this and I will mention later just how uniform it has to be.

A test location is selected, and a three-axis magnetometer is located at the center, capable of measuring fields in the B_1, B_2, and B_3 direction. These are all at right angles, of course.

This magnetometer remains at the fixed location. It is not moved during the test at any time. The flight magnetometer is removed from the spacecraft. As a matter of fact, the entire low gain antenna structure, a post about 4 in. in diameter and 3 or 4 ft long on which the magnetometer is mounted, is also removed. A short dummy tube was put in, to hold one of the instruments that is located near the

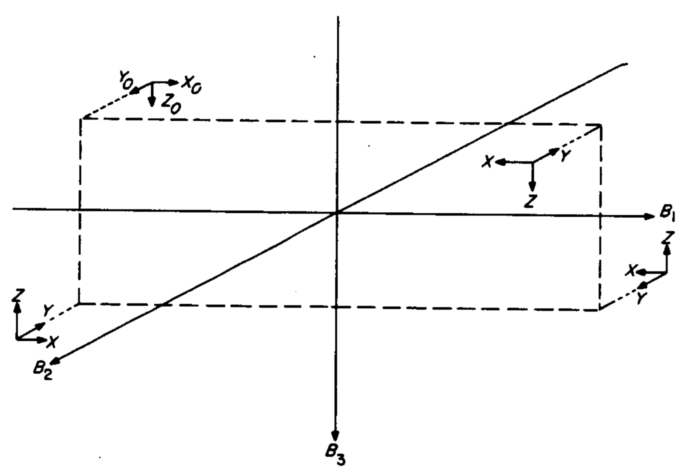

Fig. 1. Spacecraft orientations for 180 deg rotations

magnetometer. This was done to just clear the area around the location of the flight magnetometer, so that we didn't have clearance problems in moving the spacecraft around.

With the spacecraft moved a great distance away from the test area, the values of the field at the test magnetometer are measured. The spacecraft is then essentially brought into an arbitrary position at the test position. The only requirement is that the location on the spacecraft of the flight magnetometer is at this origin where the test magnetometer is located. Now, assume an X, Y, Z axis system; body axes, fixed on the spacecraft, and assume for convenience in presentation that the location of the flight magnetometer is in the positive quadrant, at a +X, +Y, and +Z point. Thus, we have these X, Y, Z body axes at an initial, or zero, position at a point that is back along the $-B_1, -B_2$, and $-B_3$ directions. The flight magnetometer, i.e. the location of the flight magnetometer, is at the origin of the B_1, B_2, B_3 system. Measurements are then made with the spacecraft in four positions that correspond to rotations about the axes that are along B_1, B_2, and B_3 and that pass through the center point.

Consider the lower right position. This is a rotation about the B_1 axis. You can see that the X body axis is still parallel to the $+B_1$, and the Y and Z have been inverted. The Y axis is now along $-B_2$, and Z is along $-B_3$. The other positions are similarly derived.

The location of the origin of body axes X, Y, and Z are set at diagonal corners of a box; they are all exactly the same distance from the point on the spacecraft at which the flight magnetometer is to be located.

We now come to the basic equations. They are straightforward. The biggest problem is one of notation. You take the measurements at the four locations and combine them in an algebraic form to get the answers you want. In Eq. (1) we first have the total field as measured in the 1, 2, and 3 direction. In this case the spacecraft has been removed from the test location, so we measure the B_1, B_2, and B_3 fields that are the Earth's inducing field. For convenience, I break the measurements up into a constant part, C_1, C_2, and C_3, and a variable part. The M quantities are the ones that will vary during the test.

$$T_1 = 1B_1 + 0 + 0 = C_1 + M_1 \tag{1a}$$

$$T_2 = 0 + 1B_2 + 0 + 0 = C_2 + M_2 \tag{1b}$$

$$T_3 = 0 + 0 + 1B_3 + 0 = C_3 + M_3 \tag{1c}$$

We bring the spacecraft into the initial position (Eq. 2), and now we measure the total field in the three directions. The numerical subscript zero preceding the letter indicates this is the initial or zero position. The induction coefficients, the a_{11}, a_{22}, and so on, are involved. These are due to the presence of soft magnetic material.

$$_0T_1 = (1 + a_{11})B_1 + a_{12} B_2 + a_{13} B_3 + P_1 = C_1 + {_0M_1} \tag{2a}$$

$$_0T_2 = a_{21} B_1 + (1 + a_{22})B_2 + a_{23} B_3 + P_2 = C_2 + {_0M_2} \tag{2b}$$

$$_0T_3 = a_{31} B_1 + a_{32} B_2 + (1 + a_{33})B_3 + P_3 = C_3 + {_0M_3} \tag{2c}$$

For the initial position of Eq. (1), the 1, 2, 3 subscripts of P_1 and a_{ij} are the X, Y, Z axes of the spacecraft

The Ps are the three perm fields that are associated with hard perm magnets, and they come in, in this fashion. All the signs are defined positive, and again we break the measurement down into these same constants, Cs, and into the variable quantities, Ms.

For the initial position of the spacecraft as at this given orientation, you may identify the subscripts 1, 2, 3, of the induction coefficients directly with the a_{ij} of the X, Y, Z axes of the spacecraft. In other words, a_{11} is a_{xx} because we have aligned the spacecraft so that the X axis was along the B_1 direction. Similarly, for the perms, P_1 is P_x, etc.

What we need to do is to express the fields that are measured at the three rotated positions in terms of these same a_{11}, a_{22}, and perms. In Eq. (3) we do this by considering a reversal of fields and measurements. Consider a case in which we reverse the direction of fields B_2 and B_3; therefore, the signs of these two terms reverse. Also, we reverse the direction of measurement along B_2 and B_3 so that we have minus signs in front of the two brackets. We do not change the X axis measurement.

$$+ \left\{(1 + a_{11})B_1 - a_{12}B_2 - a_{13}B_3 + P_1\right\} \tag{3a}$$

$$- \left| a_{21}B_1 - (1 + a_{22})B_2 - a_{23}B_3 + P_2 \right| \tag{3b}$$

$$- \left| a_{31}B_1 - a_{32}B_2 - (1 + a_{33})B_3 + P_3 \right| \tag{3c}$$

Now, if you look at what the actual field configuration is for these ficticious changes, you will find that it is exactly equivalent to rotating 180 deg around the B_1 axis. Remove the minus signs and clear up the equations and they give the fields along the 1, 2, and 3 axes after the spacecraft has been rotated about the number 1 axis (Eq. 4).

$${}_1T_1 = (1 + a_{11})B_1 + -a_{12}B_2 + -a_{12}B_3 + P_1 = C_1 + {}_1M_1 \tag{4a}$$

$${}_1T_2 = -a_{21}B_1 + (1 - 2_{22})B_2 + a_{23}B_3 - P_2 = C_2 + {}_1M_2 \tag{4b}$$

$${}_1T_3 = -a_{31}B_1 + a_{32}B_2 + (1 + a_{33})B_3 - P_3 = C_3 + {}_1M_3 \tag{4c}$$

Similar equations for ${}_2T_i$ and ${}_3T_i$

Similiary, there are two more sets of three equations corresponding to rotation about the B_2 axis and rotation about the B_3 axis. We then have five sets of three measurements each. We now go to Eq. (5)in which we consider only those measurements that were made along the number 1 axis.

$$T_1 - C_1 = M_1 = -C_1 + 1B_1 \tag{5a}$$

$${}_0T_1 - C_1 = {}_0M_1 = -C_1 + (1 + a_{11})B_1 + a_{12}B_2 + a_{13}B_3 + P_1 \tag{5b}$$

$${}_1T_1 - C_1 = {}_1M_1 = -C_1 + (1 + a_{11})B_1 - a_{12}B_2 - a_{13}B_3 + P_1 \tag{5c}$$

$${}_2T_1 - C_1 = {}_2M_1 = -C_1 + (1 + a_{11})B_1 - a_{12}B_2 + a_{13}B_3 - P_1 \tag{5d}$$

$${}_3T_1 - C_1 = {}_3M_1 = -C_1 + (1 + a_{11})B_1 + a_{12}B_2 - a_{13}B_3 - P_1 \tag{5e}$$

Equation (5a) gives the field with the spacecraft removed, Eq. (5b) with the spacecraft in the initial position and the others with it rotated about the 1, 2, and 3 axes. I have brought the C_1 term to the left so that we are looking at the variable part only. If you look at the array, you see the changing sign pattern that permits you to separate the various quantities. For example, if we add ${}_0M_1$, ${}_1M_1$, ${}_2M_1$, and ${}_3M_1$ (Eq. 6); the B_2, B_3, and P_1 terms drop out and we have $4(-C_1 + B_1 + a_{11}B_1)$. We now subtract $4M_1$ and arrive at the quantity $4a_{11}B_1$. It is a linear combination of the measured, M, quantities.

$$-4M_1 + {}_0M_1 + {}_1M_1 + {}_2M_1 + {}_3M_1 = 4a_{11}B_1 = 4a_{11}(C_1 + M_1) \tag{6a}$$

$${}_0M_1 - {}_1M_1 - {}_2M_1 + {}_3M_1 = 4a_{12}B_2 = 4a_{12}(C_2 + M_2) \tag{6b}$$

$${}_0M_1 - {}_1M_1 + {}_2M_1 - {}_3M_1 = 4a_{13}B_3 = 4a_{13}(C_3 + M_3) \tag{6c}$$

$${}_0M_1 + {}_1M_1 - {}_2M_1 - {}_3M_1 = 4P_1 \tag{6d}$$

The Cs have dropped out. They are constant quantities and large compared to the Ms. It is required that they be constant and stable to a high degree, about 1 part in 10^5. However, they do not have to be measured because they do drop out in the data reduction. Typically, the M measurements were made to 1 part in 10^3. By the other linear combinations of the zero, one, two, and three position measurements, you can break out the a_{12}, the a_{13}, and the perm quantities. A similar set of equations are used to get the other sets, the a_{22}, and so on.

This is the test procedure, it involves making a measurement with the spacecraft removed from the area to measure the B fields, and then bringing the spacecraft into four particular locations, defined in Fig. 1, and related by 180 deg rotations about axes that pass through the location of the flight magnetometer.

Now, all of these equations are certainly based on perfect magnetic materials. The a_{ij} are the result of infinitely soft permeable material with constant permeability independent of the strength of the inducing field and the P_i, represent infinitely hard permanent magnets with values or strengths that do not change. Obviously, these conditions do not exist in reality, and we have to see under what circumstances we are permitted to make this kind of a mapping measurement.

I am sure that any of you who have worked in the magnetic field area will recognize Eq. (7) as that straightforward equation describing the on axis induction produced by a sphere of radius b placed in a uniform field.

$$B_r = B_o \left[1 + 2 \frac{b^3}{r^3} \frac{\mu - 1}{\mu + 2} \right] \tag{7}$$

$\frac{b}{r}$	$\frac{1}{3}$	$\frac{1}{4}$	$\frac{1}{6}$	$\frac{1}{9}$	$\frac{1}{12.5}$
$2\left(\frac{b}{r}\right)^3$	0.07	0.03	0.01	0.003	0.001

The measured quantities for the Mariner Mars spacecraft were at the 0.001 level and lower. The maximum value of a_{11} was 0.0006 as you will see in Fig. 7; the values of the others were considerably less. Thus, we are at the point where these induction coefficients will not distort the interplanetary field. For this item, it won't make any difference whether the permeable material is present or not; the Mariner Mars instrument would not resolve this small an increment in the field.

Now let us put some dimensions on the spacecraft. We have a flight magnetometer essentially located 4 ft above the base of the hex structure that makes up the lower part of the spacecraft. Most of the magnetic components were located in the hex. On this basis, a 0.001 induction coefficient is represented by a sphere of something like 4 in. in radius. That size sphere of continuous, high permeability material would cause an induction of 0.001 at the location of the flight magnetometer. The μ - 1 over μ + 2 term is neglected, i.e. μ must be only high enough that this term is essentially unity. The sphere need not be solid but must not be too thin if its permeability is not reasonably high.

What we are concerned about is that we are going to do this mapping in the Earth's field. We should be concerned about the fact that in the Earth's field whatever permeable items on the spacecraft that caused the actual measured coefficient of 0.001 might change their permeability in interplanetary space. It might be very close to unity there, because the inducing fields in interplanetary space are very, very small.

We are not concerned about the lack of distortion of the interplanetary field, as we have shown. We are concerned about the lack of distortion of the fields

produced by the permanent magnetic material on the spacecraft. In other words, in a measurement made in the Earth's field we may not measure the right perm fields at the magnetometer location. To examine this we may represent the small permanent magnet as a current loop and find out the effect the permeability of a sphere has on the field produced by the loop at some distance.

The same type formula, at least for a first approximation, applies where we now consider that the little permanent magnet is located at a distance a from the sphere of radius b. The formula applies for r larger than a which is larger than b.

$$B_r \approx B_o \left\{1 + 2\left(\frac{b}{a}\right)^3\right\}$$

Now, as a matter of fact, on the spacecraft, those permeable items that had reasonable size will probably maintain their permeability. For example, consider the motor-driven switch that a shield can was put around to reduce the stray field. The flux density in the shield can was already up to where this material had a reasonable permeability. It had to be so to provide the shielding, and this induction is supplied by the internal magnet. Items of this size and kind (I understand this particular switch was about a 3 in. cube) probably will not lose their permeability in space. So, what we are really dealing with is a large number of probably very small permeable items that might not exhibit the proper permeability under the low field conditions.

Now, what we are concerned with is that if you say you only want the effect at some distant point, i. e. at the magnetometer, to be disturbed only 1% by presence or absence of permeable magnetic material, it is required that the b/a ratio be something like a sixth. This means if this sphere was 1 in. in radius and if a permanent magnet 6 in. away is contributing to the field at the magnetometer sensor, then if that small permeable sphere were to change from some large value to nearly unity permeability, there would only be a 1% change in the effect at the magnetometer location. This is all I wish to say about this; it is not a rigorous proof of anything but I just present it to you, frankly, to see what effect it might have.

The other item I wish to mention has to do with the uniformity of the inducing magnetic field, the Earth's field, over the region in which the spacecraft is to be mapped. What is desired is that this field can be absolutely uniform so when a permeable body induces a field at some point, the induced component when the body

is moved 180 deg about this point is the same. The same kind of formula applies at least to first order. Some gradient can be tolerated and we wish to estimate its value.

To do this, we place a small current loop at a large distance, a, from an origin to provide a gradient in the region near the origin. Then we find out how large this gradient may be and yet keep the difference in the fields induced by a sphere when it is moved from one side of the origin to the other down to some quantity. Again let the radius of the sphere be b and the distances from the origin, r. We desire the difference in the induced fields to be some number such as 1 gamma. The inducing field must be taken as about equal to the Earth's field; we use 50,000 gamma. We use a b/r ratio equivalent to what would be measured on the spacecraft. When we put the numbers in the formula, it turns out that the average gradient over the distance must be about 130 gamma/ft.

Now, we will move on to the next item, the fixture itself. To do its job, it certainly had to be completely nonmagnetic. It was to be made out of materials that were nonmagnetic; at least we felt we could control that part of it. For example, it was hand-powered. We made no attempt to put any kind of actuators or motors or drives on it. We had a man turn the crank and step back after he moved it from one position to another.

Next, another truly important operational requirement was that it be easy to operate. You must be able to move the spacecraft from one position to another reasonably rapidly. We are talking about cranking speeds on the order of 1 or 0.5 rpm. We aren't considering moving instantaneously from one position to another. As a matter of fact, because of the motion itself, eddy currents are generated in the structure that require some fraction of a second to die out. The time scale of operation is on the order of many seconds.

We were asking for reasonable mechanical repeatability. We wanted to be able to repeat these positions. We didn't need fantastic accuracy such as rotation about a perfect circle, or anything of that kind. After all, the magnetometer sensor is a finite volume of about 1 in^3. We didn't expect significant gradients over that region.

The mechanical engineering cognizance was given to our engineering mechanics division as is the standard practice. Mr. Robert Norman was assigned the cognizant engineer's responsibility. Sperry, Utah, was the contractor; they did an excellent job on short notice and with a very short time scale for accomplishing it.

The people from the Spacecraft Assembly Facility area, under Mr. Goldfine, participated in the design. These were the people that were going to have to move the spacecraft and be responsible for its handling. Their practical comments were invaluable.

As I said earlier, there was reference made to a king-sized "lazy Susan." Well, this turned out to be a real king-size one as shown in Fig. 2.

One requirement was that this device be completely portable. This is shown in its knocked-down or shipping position. It was made to be loaded aboard a couple of vans, and could then be moved over the highways without any unusual problems. What you are seeing here are a couple of sections of the flat bed. The distance across is on the order of 15 ft or so.

Figure 3 shows it in the assembled position where we have the circular track for rotation about the vertical axis and the big framework that actually rotates. There is a hinge point that permits the spacecraft mounting or adapting ring to be put in a horizontal position for loading the spacecraft and, then, after it is loaded, the whole assembly is rotated up in a horizontal position so that the roll axis of the spacecraft is horizontal.

The magnetometer test fixture is located at what is going to be the location of the flight magnetometer on the spacecraft, and it is held by the massive tubular structure. Even though the magnetometer is only 1 lb this was necessary for rigidity. The tubular structure is nothing more than 1/16-in., or less, aluminum sheets that have been rolled into tubular structures. When it is taken down and disassembled for shipping, one person can pick up a long section, put it on his shoulder, and walk away with it.

Figure 4 shows it rotated up in the test position; the dummy antenna post is visible. The actual antenna post has the low gain antenna on the top end of it, and the magnetometer and ion chamber fasten to it at the proper locations. This is the position for one of the test mappings. The entire fixture rolls on tracks so that it can be rolled away to make the initial measurement that is required to get the Earth's fields at the point of the test magnetometer.

This is an assembled spacecraft with essentially everything but the solar panels. If we had tried to put them on, I guess the fixture would have had to grow at least 12 ft. In their place are small aluminum racks or frames that were required for support of the gas control system that normally is on the solar panels — some of the valves and jets and so forth were assembled as part of the spacecraft, and that

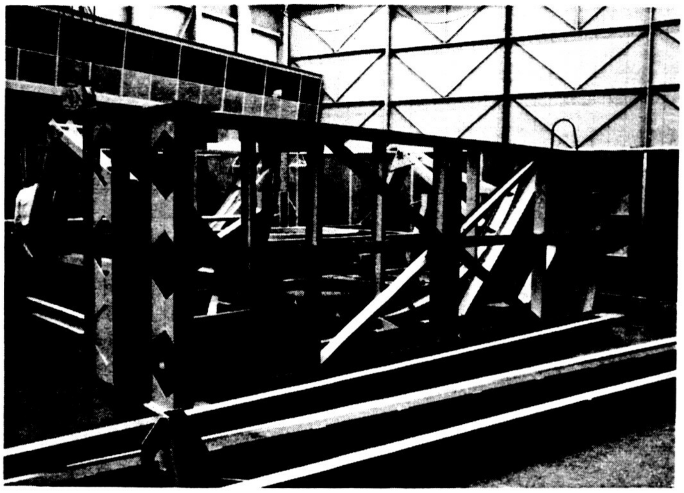

Fig. 2. Spacecraft mapping fixture folded for shipment

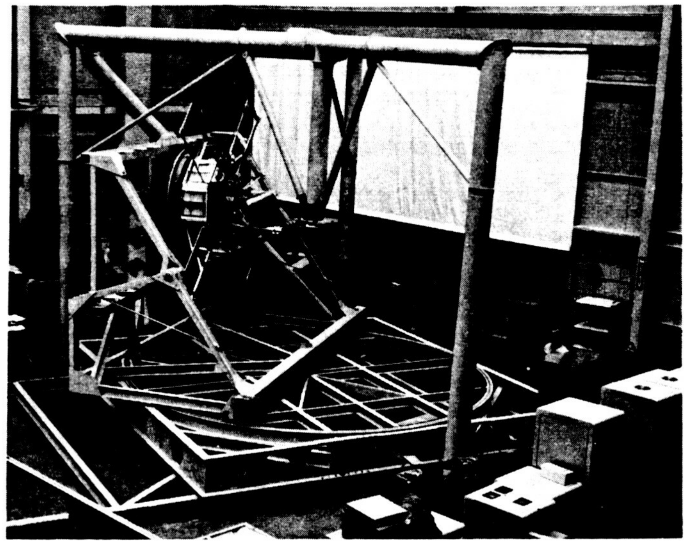

Fig. 4. Spacecraft mapping fixture in test position with Mariner Mars mounted

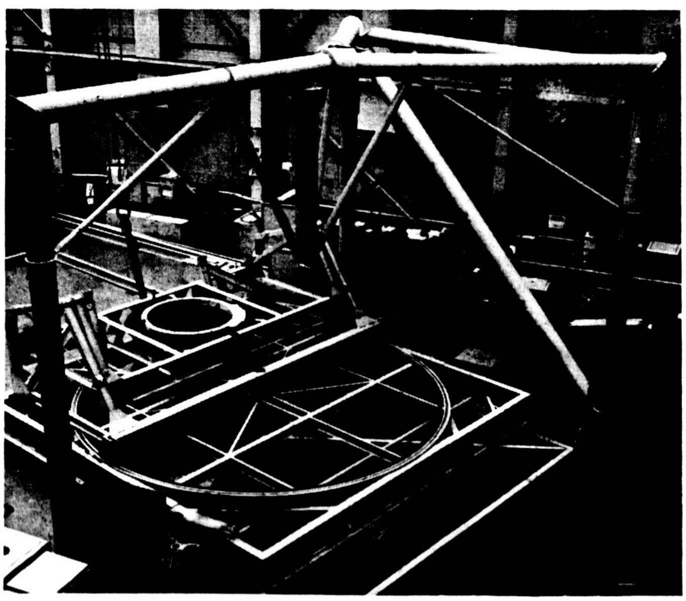

Fig. 3. Spacecraft mapping fixture assembled for use

joint could not be broken. These things, generally, were made of permeable material. In the normal folded-up position, these items would have been very close to the magnetometer. So, we required that they provide these kinds of fixtures so we could locate these pieces of hardware equipment at their actual flight locations.

Figure 5 is another view of the assembly showing the test magnetometer, shortened dummy antenna, and ion chamber.

As a last item, we will take a look at a couple of the curves that summarize some of the data that was obtained by mapping.

Figure 6 gives the values of the permanent magnet field along the X, Y, and the Z axis. We used four sets of initial positions that corresponded to 45-deg increments in initial positions. These are the values of the perm fields as measured by this process, representing values distilled from four sets of four measurements. You can see the values are quite reasonably constant over the entire experiment.

Figure 7 gives some of the induction coefficients. a_{11} happened to turn out to be fairly constant; a_{22} and a_{33} showed some cyclic variation from the various tests (Fig. 7a). But recognize, these are in fairly small quantities and the fields associated with them are fairly small.

With a_{12}, a_{13}, and a_{21} (Fig. 7b), we are down to 10^{-5} values. At this level, we don't know just what caused these variations. These numbers, ideally, should all be a straight line, but there are some secondary effects that we didn't unravel.

OPEN DISCUSSION

MR. PARSONS: What was the stability of your background level in that setup that you were in, there?

DR. STALLKAMP: We were monitoring the Earth's field from other magnetometers located in adjacent areas. In the actual test procedure, after we rotated 360 deg, we are back to the same configuration we start with and we are able to compare values, — make a closure point. The closures were better than 1 gamma. It took something like 10 or 15 min to rotate through one of these sequences, so that's a measure of the time stability of the Earth's field at that time, at this place, and under these conditions. They were all done at night or the early hours of the morning.

MR. PARSONS: I would say that you had remarkable good luck. What was the calendar date that these tests took place?

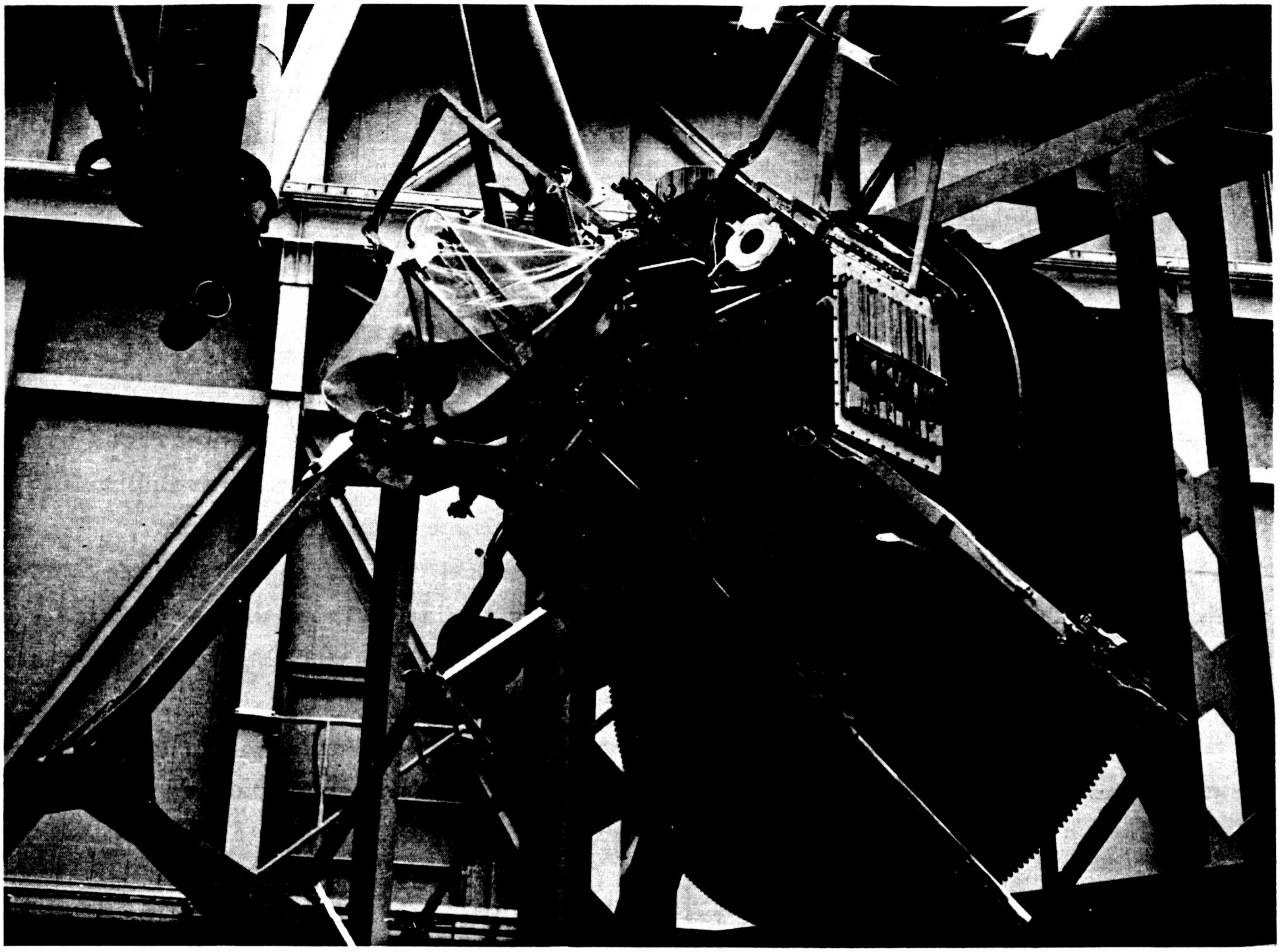

Fig. 5. Spacecraft mapping fixture in test position, closeup of test magnetometer

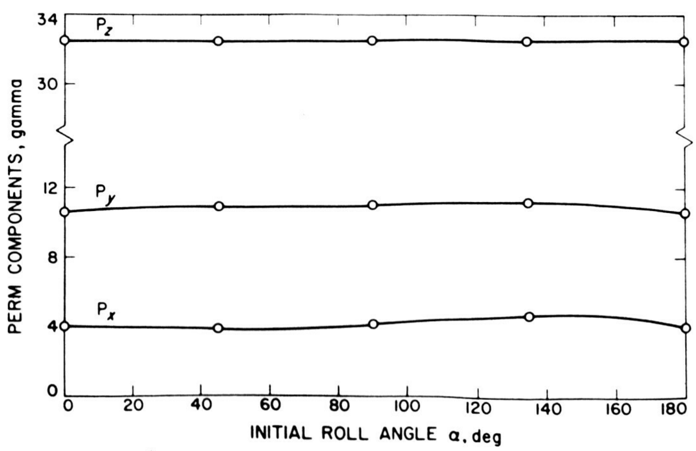

Fig. 6. Mariner Mars permanent magnetic fields

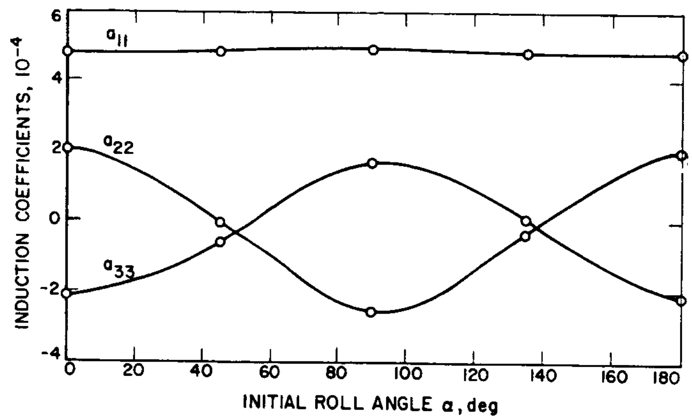

Fig. 7a. Mariner Mars magnetic induction coefficients

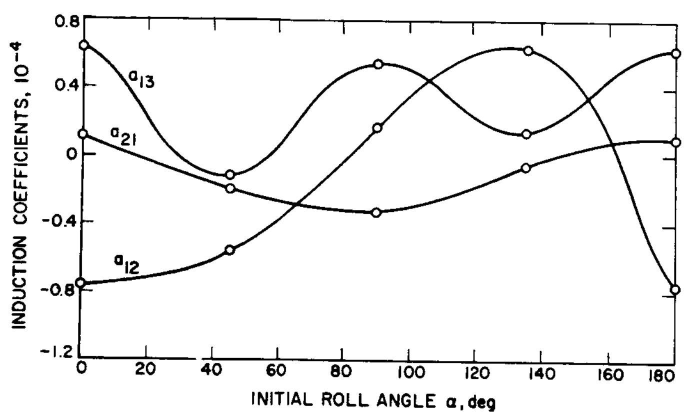

Fig. 7c. Mariner Mars magnetic induction coefficients

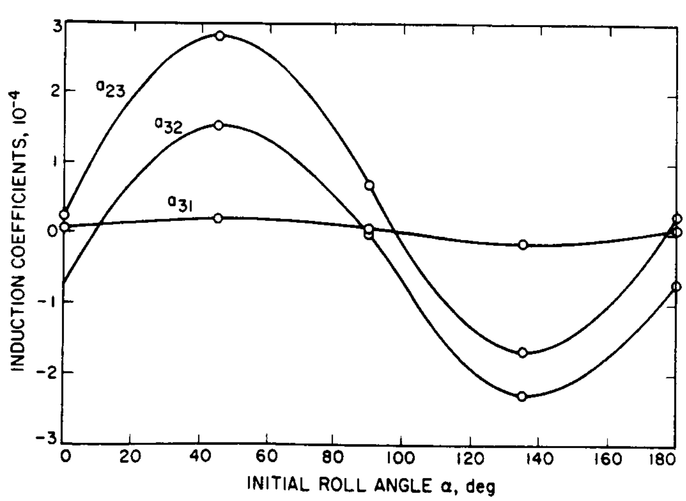

Fig. 7b. Mariner Mars magnetic induction coefficients

MR. FRANDSEN: I guess last May was the first mapping, and then at various times until October, at the Cape. They were all run at night starting at somethink like 1:00 a.m. and continuing until dawn. The whole building was shut down, including the traffic and machines. In a 20-min period, we could get very good closures — within 1 gamma. There were some with 4 or 5 gamma, and we would just repeat the procedure until we got good closures.

DR. STALLKAMP: If something changed, you started it over. We were able to get tests where the closures were below 1 gamma.

VOICE: Were you monitoring the background?

DR. STALLKAMP: Yes, in general, we were. I believe that is was constant within these same values. There was a separate magnetometer sensor in the area, but certainly out of the range of the influence of the spacecraft.

MR. LYNCH: When you monitor the background, to what accuracy do you do this?

MR. FRANDSEN: That's an embarrassing question. I used a rubidium total field magnetometer; it was on the order of, maybe, 100 gamma full-scale on the Sanborn recorder. That's 50 divisions, so I suppose that that is about 2 gamma per division in our monitoring probe. It was still a reasonably stable field for the test, otherwise, we'd just start over again. So, it was quiet during the period of the tests.

MR. CHRISTY: I would like to add to what Mr. Frandsen just said. Most of the time, the rubidium was monitored intermittently regularly through the test with a digital voltmeter. As I recall, these readings were extremely close, indicating, perhaps, as good as 0.5 gamma or less during the period of one test cycle.

MR. FRANDSEN: You refreshed my memory. That's right. We also had the counter of the rubidium magnetometer and the counter was counting frequency — the one we had was 5 cps/gamma. So, we could count on the counter to 0.2 gamma, you might say. Typically it was staying within five cycles for the test for which we used the data. Admittedly, there were times that it went out of these bounds. We tried to wait a little bit. Possibly, even if it had gone 10 cycles or 2 gamma, we would have used it but, by and large, it was reasonably quiet at that time of night. Also, at times we ran the test even faster. We just took the essential four data points and this gave us even better closure.

MR. PARSONS: Did you measure the gradient of the field across that 15-ft volume and, if so, how much was it?

DR. STALLKAMP: Yes, we did. As I recall it, it was on the acceptable order of 130 or so gamma/ft.

MR. PARSONS: Is this based on this calculation?

DR. STALLKAMP: Yes. It was about 100 gamma/ft in our high bay SAF Building.

MR. FRANDSEN: This was steel-reinforced concrete.

DR. STALLKAMP: But you are away from the walls.

MR. FRANDSEN: We were up pretty high and we measured with the rubidium; it was a total field reading. The total of X, Y, or Z, and it was on the order of 100 gamma/ft, which is high, but by this calculation is acceptable.

MR. WOOLLEY: I have two questions. First, did I understand that you did not measure the three components of the field independently in measuring the field gradient? You were just taking the total field at various points across there?

DR. STALLKAMP: Yes, just to establish the suitability of the test location.

MR. WOOLLEY: Secondly, if I understand correctly how you defined your induction coefficients, I don't understand the significance of those negative induction coefficients. In two axes you had them about as far negative as often as you had them positive.

DR. STALLKAMP: In the final graphs that I showed?

MR. WOOLLEY: Right.

MR. FRANDSEN: I think what it means is, for example, if you had an a_{12} that was negative, you would say that the ambient field in the one direction contributes to the minus two direction of the probe. It is just a matter of which direction does the induced field contribute to the flight probe. If the ambient is along the spacecraft +X axis, it might contribute to the -Z probe axis. You have a minus sign there.

MR. WOOLLEY: How do you interpret a reversal of induction coefficient?

MR. FRANDSEN: As Dr. Stallkamp said, if it were the ideal model, they would all be straight lines. This reversal might be a geometric factor. I don't really know.

MR. WOOLLEY: Well, I could understand a variation as a geometric factor, or a disturbance from the steel surrounding it, but I don't understand a reversal as a geometric factor.

DR. STALLKAMP: Those curves should have been straight lines. You can certainly have a negative value for these coefficients. There is no problem there. These reversals are one of the anomalies that showed up. The curves represent at least four separate tests with different initial orientations. They were taken

concurrently and, as I said, if the linear approximation holds, that number should have been constant; because it wasn't as a defect in the test. Again, note we are getting down to where we are subtracting small quantities. When these numbers got down to 10^{-5}, we are down to a few gamma of field. It is, admittedly, a defect that that we don't understand.

MR. PARSONS: Did you manipulate the fixtures without the payload in place?

DR. STALLKAMP: Yes, we did. They were completely nonmagnetic. There was less than 1 gamma change rotating around either axis. The Sperry people, who contributed considerably to the basic design, did a very good job. They made it light by taking aluminum I-beams that were about 12-in. in depth and 12-in. in flange width, cutting them in that hexagonal pattern, moving them over a half a notch, and then welding them together to get a 24-in. deep beam with a third of the weight, or something of that kind. They were able to assemble it, bolting it together, so that it came out flat enough so no excessive machining was necessary on the surfaces where the rollers operated. It came out a very satisfactory job.

The size of it, of course, was dictated by the basic size of the Mariner Mars spacecraft and the location of the sensor on the antenna post. The main center of gravity of the craft is back somewhere in the hex, or certainly close to that. We wanted to be in a situation in which we were completely gravity-stable in moving it around, so we simply let the fixture grow up to where the center of gravity remained inside the load carrying members and surfaces. As a result, we were able to move the thing rapidly, easily, and with confidence that we weren't going to hurt something.

VOICE: Did you do any calibration with just a standard piece of metal in the fixture?

DR. STALLKAMP: I believe a little of that was done, I don't recall for certain. I think we just went right ahead and did the actual tests. We were committed to this, for one thing, but there was some experience with the Mariner II spacecraft — they had used this same technique and with a fixture that they had put together very rapidly. I understand it was pretty hard to operate. We just went ahead, that's true.

N66-11283

RESONANCE TECHNIQUE FOR MEASUREMENT OF SATELLITE MAGNETIC DIPOLE MOMENT

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INTRODUCTION

It has long been a problem to accurately determine the magnetic characteristics of a body. Methods to carry this out may measure a body's magnetic field and construct a correlating dipole moment. If the body is large and its field relatively low, however, magnetometers placed nearby necessarily see near fields and their higher order poles. A more direct approach is to measure body torque reactions to known fields. In a fixed field, the steady torque produced is often no greater than atmospheric disturbances and system accuracy is impaired.

This paper presents a novel practical approach to the measurement of dipole moments, their strength, and possible origin. The method is called the pulsed resonance technique and was suggested by F. F. Mobley. The technique uses a square wave field oscillating synchronously with the torsion suspension body resonance. The deflection amplitude increases in time and provides direct measure of a single axis dipole moment. Three-axis testing defines the complete payload; or, taking a single axis at a time, it is possible to negate all dipole moments.

This paper discusses, in particular, the application of this technique to the z-axis dipole moment alignment of the NASA BE-C satellite. The mathematical theory of pulsed resonance involves some Fourier and Laplace methods, and leads directly to a closed solution and error analysis. With a little care in setting up the experiment it can be shown that the method provides an accurate determination of dipole moments. The ultimate accuracy of this system has not yet been achieved by this author. It is suggested that it be adopted by others in the field.

PULSED RESONANCE TECHNIQUE FOR MEASURING DIPOLE MOMENTS

Theory

It is assumed that a rigid body exists that has components M_x, M_y, M_z of magnetic dipole moment directed along body fixed x, y, z axes, respectively. The body is suspended by a torsion wire along the z-Z axis so that the single degree of freedom is, θ, the body rotation in a horizontal plane. The suspension torsional

stiffness, k, and body moment of inertia, I, are so adjusted that a well defined natural period of oscillation, τ, exists (30.00 sec, for example). Torsion wire and air damping at such τ can be neglected.

A single horizontal magnetic field component, H (t), is generated in the form of a square wave with a zero-to-peak amplitude of h and period τ, i.e. in resonance with the free body period (Fig. 1). The field is such that only cosine functions exist in the Fourier series representation.

The body torque $\overline{T} = \overline{M} \times \overline{H}$ is restricted to the X-Y plane; it is further assumed that the body rotation remains small so that only the dipole component normal to the field brings about a reaction torque to H (t). The time response solution is given by an equation of the form

$$\theta(t) = \frac{4hM}{\pi I \omega_\mu^2} \left[\frac{\omega_\mu t}{2} \sin \omega_\mu t + \frac{1}{24} (\cos 3\omega_\mu t - \cos \omega_\mu t) - \frac{1}{120} (\cos 5\omega_\mu t - \cos\omega_\mu t) + \frac{1}{366} (\cos 7\omega_\mu t - \cos\omega_\mu t) \cdots \right] + \theta_0 \cos\omega_\mu t + \frac{\theta_1}{\omega_\mu} \sin \omega_\mu t \tag{1}$$

where θ_0 and θ_1 describe initial conditions of displacement and velocity, respectively.

By means of an optical lever, the time response is followed on a screen. The position is marked at every field reversal, i.e. for $\omega_\mu t = (N-1/2)\pi$ where N is a nonzero positive integer. These positions, although not actual deflection peaks, define the linear growth of θ as

$$|\theta(N)| = \frac{2hM}{\omega_\mu^2 I} (N-1/2 + \rho) \tag{2}$$

where

$$\rho = \frac{\theta_1 \omega_\mu I}{2hM} .$$

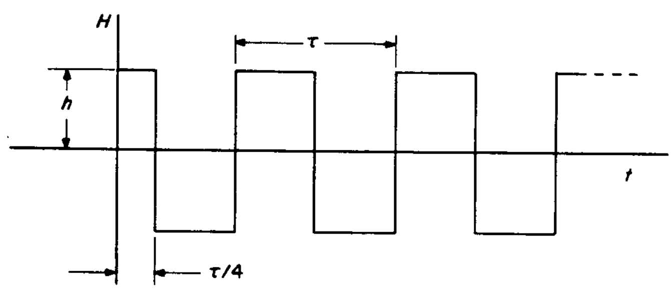

Fig. 1. Magnetic field vs time

The dipole moments themselves are measured from Eq. (2) as

$$M = \frac{2\pi^2 I}{\tau^2 h} \frac{d\,|\theta(N)|}{dN} \tag{3}$$

Calibration

A calibration test was performed on a mockup BE-C satellite on 26 February 1965, at the Naval Ordnance Laboratory Magnetic Testing Facility. The purpose of this test was to determine the model dynamic response characteristics and to calculate the M_x and M_y of known trim magnets. The model was suspended upside down in the coil system center according to Fig. 2. The calibration model parameters are listed in Table 1.

Table 1. Calibration model parameters

Suspension	
4 strands no. 23 steel wire	
Diameter	$d = 0.051$ in.
Length	$L = 72.4$ in.
Spring rate	$k = 0.426$ in. -lb_f
Body	
Moment of inertia	$I_z = 0.810$ $\text{ft-lb}_f\text{-sec}^2$
Z-axis dipole moment	$M_z = 8.96 \times 10^4$ pole-cm
System	
Natural period of oscillation	$\tau = 30.00 \pm 0.05$ sec
Optical lever length	$R = 26.80$ ft
Magnetic field zero-to-peak	$h = 0.400$ oersteds

An optical lever system was used to measure both period and deflection. The period measurement technique is presently being used to accurately determine payload moments of inertia. A converging mirror is attached to the body so that a thin rectangle of directed light is focused on a solar cell covered by a similarly narrow aperture. The solar cell output triggers an electronic timer. By triggering both the time start and stop to the pulse maximum, 0.02% accuracy can be obtained. To

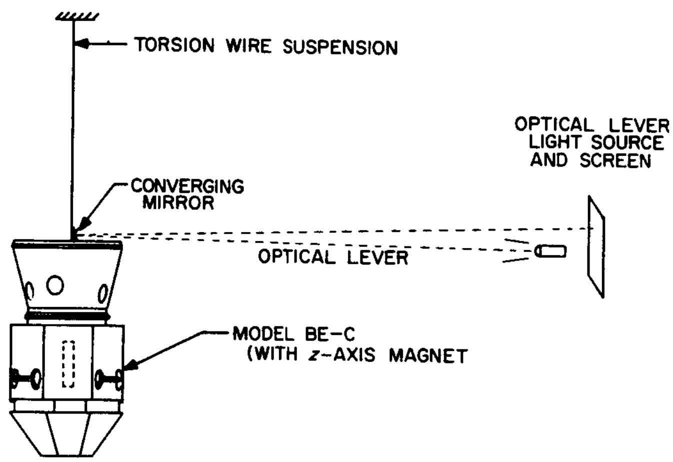

Fig. 2. Calibration model
test arrangement

measure θ, the mirror focused a reticle some 8 m from the body. The image translation is $\xi = 2R\theta$ where R is the optical lever length.

Figure 3 plots ξ versus N and illustrate experimental response linearity. Runs numbered 1a, b and 2a, b measure the calibration body initial M_x and M_y, respectively. From Eq. (3) these moments were determined as $M_x = -325$ and $M_y = +580$ pole-cm. A trim magnet of $\Delta M_y = -569$ was added for run number 3 leaving a theoretical residual of $M_y = +11$; the measured residual was $M_y = +102$. A second trim magnet was added for run number 4 producing a theoretical residual of $M_y = -685$; the measured dipole for this case was $M_y = -613$. Because of an error in the initial evaluation of M_y, it is not unreasonable to state that the calibration system sensitivity is on the order of 50 pole-cm. It should be noted that this value is equivalent to a 1.9 min wobble angle of the z-axis magnet.

During runs number 5 and 6, the trim magnet $\Delta M_y = -696$ was removed and a 2.92 lb bundle of 10.5 in. long annealed AL 4750 hysteresis rod material was positioned 45 deg to the x-axis. The measured dipole moment change of $\Delta M_x = \Delta M_y = +42$ pole-cm probably was due to the slight unbalance tilt given the z-axis magnet. Dipole effects should not be evident in magnetically soft materials during reversed pulsed resonance because field reversal also reverses the dipole moment sense. The net effect is that a constant, rather than a pulsating, torque is produced. One possible way to measure the torque created by such materials is to pulse the field between zero and full positive; this should be done, however, only after permanent transverse moments have been well defined.

Payload BE-C Transverse Dipole Moments

Payload BE-C (S/N 34) less solar panels, was suspended according to Fig. 2 on 1 March 1965. Table 2 lists experiment parameters.

Balance weights were added so that τ was adjusted to 30.00 ±0.05 sec and so the body z-axis was aligned to within 6 min of local vertical. Initial permanent dipole moments were measured as $M_x = +950$ $M_y = -93$ pole-cm. A trim magnet of $\Delta M_x = -1146$ was added; the measured change was $\Delta M_x = -1128$ pole-cm (this represents an error of 1.57%). Satellite power was turned on; the changes measured were $\Delta M_x = -31$ $\Delta M_y = -57$ pole-cm.

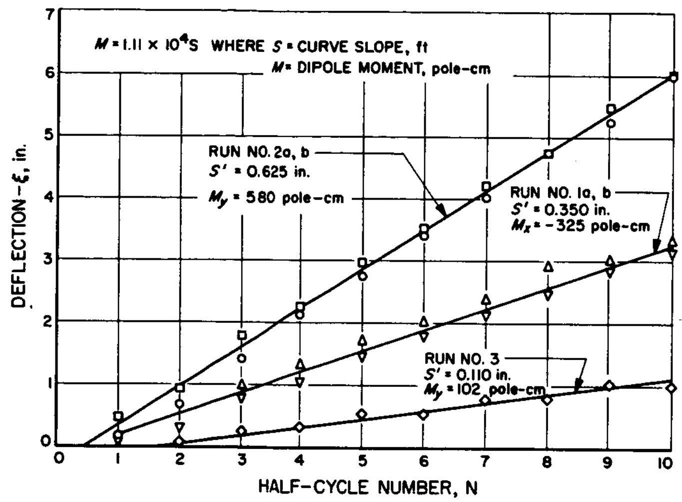

Fig. 3a. Half-cycle deflection response

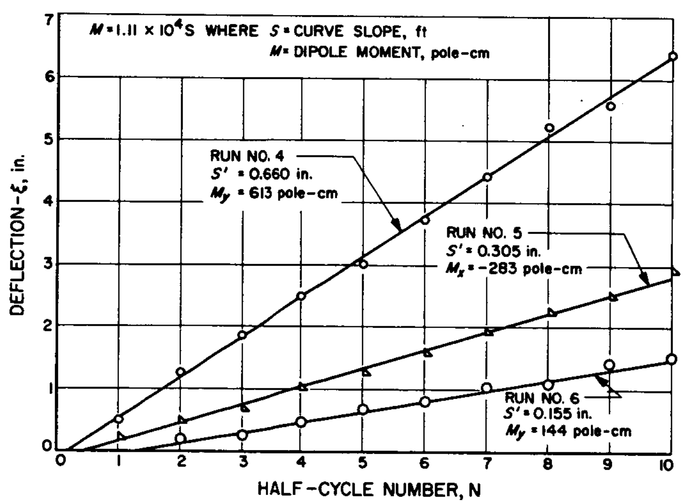

Fig. 3b. Half-cycle deflection response

Table 2. BE-C experiment parameters

Suspension	
4 strands no. 24 steel wire	
Length	$L = 70.0$ in.
Diameter	$d = 0.055$ in.
Spring rate	$k = 0.595$ in. $-lb_f$
Payload	
Moment of inertia	$I = 1.13$ ft-lb_f-sec^2
Z-axis dipole moment	$M_z = 8.70 \times 10^4$ pole-cm
System	
Natural period of oscillation	$\tau = 30.00$ sec
Optical lever length	$R = 26.80$ ft
Magnetic field zero-to-peak	$h = 0.400$ oersteds

Error Analysis

Several tacit assumptions were made during the derivation of Eq. (3). Consider their possible contribution to experimental error:

1. It was assumed that θ was small so that $\tan \theta \approx \sin \theta \approx \theta$ and $\cos \theta \approx 1.00$. This assumption produces errors no greater than 0.02% because typical deflections were under 0.020 radian.
2. It was also assumed that $H_X M_x$ and $H_Y M_y$ were $\ll k$ so that torques created by dipole components nearly parallel to the field could be neglected. Typically M was on the order of 500 pole-cm while h = 0.400 oersteds; thus MH = 200 dyne-cm while $k = 4.81 \times 10^5$ dyne-cm.
3. The evaluation of M assumes that the square wave period matches the system natural frequency. Consider, however, the forcing frequency $\omega = \omega_\mu + \Delta\omega$ and account for this deviation in the Laplace transform solution of the equation of motion. Because θ is recorded at field reversal, the solution reduces to a series in $\Delta\omega$. Defining ϵ by the percent deviation from true slope $d|\theta(N)|/dN$, the error can be resolved as

$$\epsilon \simeq \frac{\Delta\omega}{2\omega_\mu} = -\frac{\Delta\tau}{2\tau_\mu}$$

Typically $\Delta\tau = 0.05$, $\tau\mu = 30.00$ and so $\epsilon \approx 0.08\%$.

The $\Delta\tau$ here represents measured deviation in the natural period from 30.00 sec. Actually, a shift in the pulsed resonance period may be larger than this $\Delta\tau$ – predominately because of operator error. The following paragraph describes the approach used to attenuate its magnitude.

The period has been established at 30.00 sec because this allows the operator to reverse polarity as a clock second hand passes the quarter marks. Each field reversal is thus referenced to time zero rather than the preceding reversal. The longer period also provides better square wave resolution – transients at the half-period are small compared to the 15 sec dwell. Figure 4 illustrates traces of the Naval Ordnance Laboratory generated square wave field. The period standard deviation is typically 0.68 sec; deviations in field magnitude during test runs averaged 2.93% of zero-to-peak deflection. These deviations in τ and h are perhaps the most significant and constitute up to 5.2% of error.

CONCLUSIONS

The intent of this paper has been to present the pulsed resonance technique and its application to the measurement of transverse dipole moments of the NASA BE-C spacecraft. This technique is a practical approach to the measurement of dipole moments and can be used to make a complete three-axis description of permanent and current induced dipoles. An extension of the theory should lead to a method even capable of defining the arrangement of permeable materials within a body.

The BE-C problem of negating transverse moments was, of course, most difficult because wobble of the z-magnet led to errors. The technique, however, was surprisingly successful. If the z-magnet was the sole source of transverse components, it represented only 31.8 mal-alignment. Spacecraft devoid of such large magnets would more readily succumb to negating specified components of dipole moment.

ACKNOWLEDGEMENT

The author wishes to express his thanks to Mr. Baxter M. Phillips for his assistance in the experimental portions of this work.

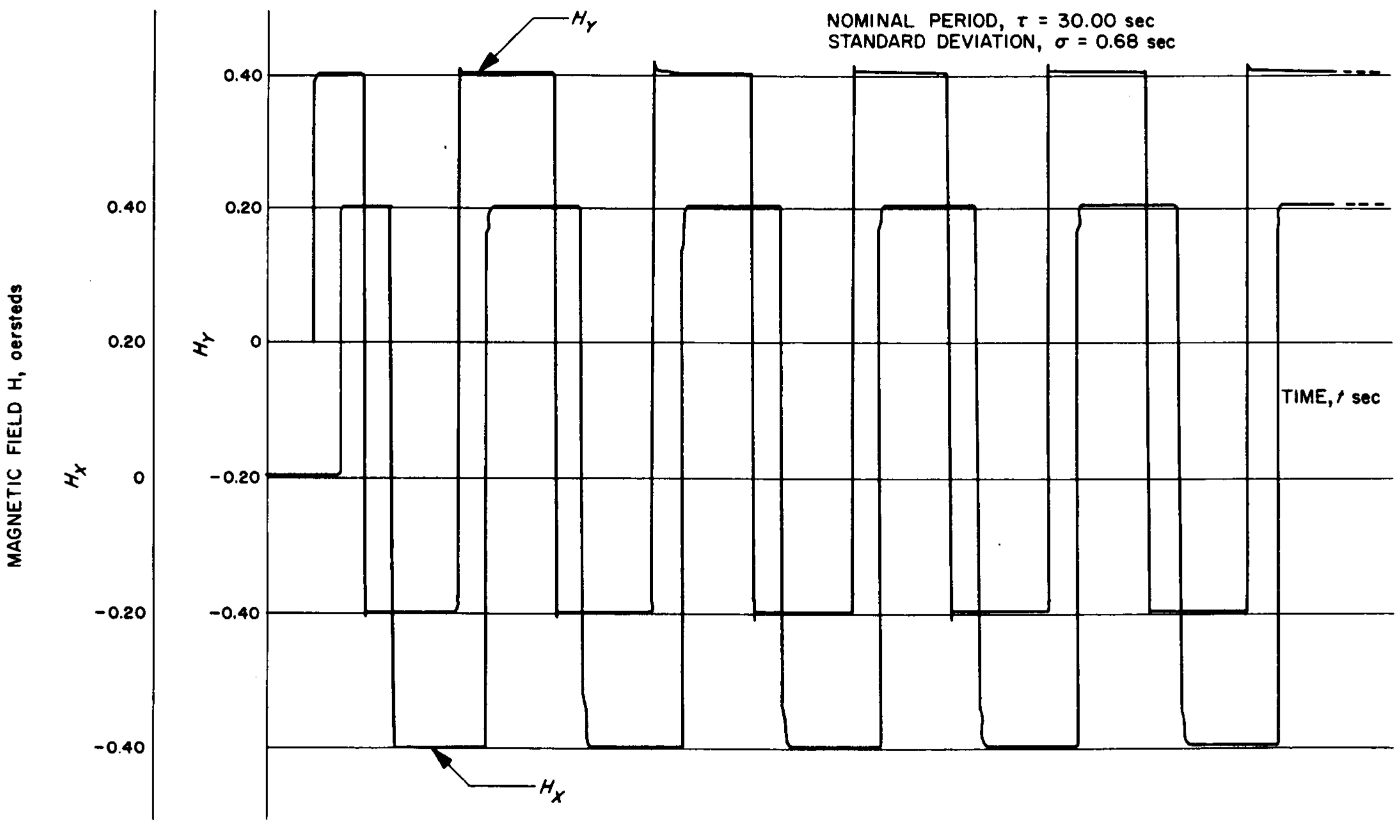

Fig. 4. Magnetic field vs time trace results

OPEN DISCUSSION

MR. WOOLEY: What sort of resolutions did you get? What size magnetic dipole moment did you get?

MR. TOSSMAN: I was measuring transverse dipole moments in the order of 600 pole-cm. During the calibration run I was getting accuracies in the order of 50 pole-cm. I did much better on the actual flight model in which a trim magnet of 1000 pole-cm was measured to an accuracy of 20 pole-cm.

VOICE: What is this unit you are using? Pole-cm?

MR. TOSSMAN: This is the cgs system. It's very hard to say what the accuracy of my system is. I think it's much better than what I measured. I ran into a great deal of difficulty, of course, because I had a very large z-axis magnet. It measured 90,000 pole-cm. So, a very small wobble of the z-magnet would contribute very large components of transverse dipole moment. If, however, there were no large magnets in the satellite, the mathematics show this technique to work very well. I see no reason why we cannot do much better.

MR. PARSONS: What was the weight of the satellite and the diameter of the wire?

MR. TOSSMAN: The satellite weighed 80 lb, I believe, this is without the solar blades. The torsion wire suspension was adjusted to the body moment of inertia to give me my 30 sec. Torsion wire length was like 4 ft, I used four wires approximately 55 mm diameter each. There was sufficient strength in the wires.

MR. PARSONS: Did you feel that air currents were a problem? Does this body have a configuration with things sticking out, or was it basically spherical?

MR. TOSSMAN: We used an octagonal structure with a conical adapter. That's the basic configuration of the satellite. This particular satellite in orbit, however, has four 4-foot solar panels attached to it, which I left off during my test.

MR. PARSONS: Are these nonmagnetic panels?

MR. TOSSMAN: Basically, they are an aluminum structure with solar cells. I was just trying to determine those permanent dipoles that existed in the satellite and also to measure the effects of current in the satellite on producing dipole moments. So, I measured the transverse dipole moments; first, with current off, and then with current on. I measured changes, I was actually able to measure

changes in the order of 30 pole-cm. I was able to see some small variations in the current-induced dipole moments that were in sequence with the cycling of certain pieces of electronics. However, I wasn't able to actually measure their magnitude.

MR. LYNCH: Could you please take some time to explain how you can extract the induced moment from the permanent moment with this technique?

MR. TOSSMAN: There may be some confusion in saying induced. I measured in my test the current induced, or shall we say the magnetic moment created, by currents in the satellite. I did not measure induced magnetic moments. I think this could be done, however, if we consider the body X-Y axes. If you have, perhaps, some linearly permeable material located off axis and generating a field, you will get a torque on the body. This may resolve itself into some sort of a dipole moment along this axis. However, if you reverse this field, the torque will probably also reverse and you will get no pulsing technique. That is why I was suggesting maintaining a field that would vary from full positive to zero and full positive again, and thus use this torque as a pulsing mechanism to drive the body into resonance, and then measure that deflection response. I have not done this yet.

MR. WEINBERGER: Mr. Weinberger, General Electric. I am wondering how you calibrated your torsion wire, and did you take into account, perhaps by your assembly, that you have four parallel strands that might give you a different torque constant from the individual wires?

MR. TOSSMAN: I did run into problems with this, initially. I really don't care as long as the spring rate is linear for small oscillations. So long as the period of the body in its torsion suspension matches exactly that period I am using as the fundamental of the field. Yes, depending on what type of suspension you get, you can have the bifilar effect. I actually used this to my advantage, because I set it up having the wires separated by a small distance — say 0.25 in. There were two wires. There are a total of four wires through two eyebolts. I just wrapped the wires through, and this will give you a definite bifilar effect by drawing them together near the top, and near the bottom you actually can get some adjustment of the spring rate.

This adjustment plus balance weights enables you to get whatever period you want.

MR. WEINBERGER: Are you saying that your technique is independent of knowing the exact magnitude of the spring rate? In other words, the spring rate washes out.

MR. TOSSMAN: We found out presicely what the moment of inertia of the satellite was. From that, and knowing precisely the period of oscillation, the spring falls out. So, in this particular case, I really didn't care what the spring rate was.

MR. WEINBERGER: In other words, you didn't check the individual wires? You did this in the assembly?

MR. TOSSMAN: Oh, yes. I prescribed my lengths and diameters of the wire to give me my 30-sec period for the known moment of inertia of the satellite; however, when you set it up, there are some deviations from this 30 sec. I assumed no bifilar effect when I initially set it up, and I eliminated it by taping the wires together. So, instead of going through a wide loop like so, they were actually forced to converge. Over 90 or 95% of the length you have just one small bundle of wires. The torsional constant of that then being four times the spring rate for a single wire.

MR. BROOKS: Mr. Brooks, Republic Aviation. Did you run any tests in spinning the satellite at a right angle to the way you did it this way – suspending it on a hook located at the cg?

MR. TOSSMAN: I did not on these tests; however, we do have all sorts of problems in attitude control where I am going to have to do this for other satellites. This technique can be extended, yes, to measuring, first the transverse dipole moment in a horizontal plane, and then tilting the satellite and measuring the – well, piecing together the total vector dipole moment. You would just have to use suspension other than what I have done here. Perhaps a large platform with four wires on the edges – say, a quadfilar effect perhaps. It appears simple enough.

MR. PARSONS: What was the angle of rotation that you worked with?

MR. TOSSMAN: I was working with θ in the order of 0.02 rad; extremely small. I was measuring the deflections 8 m away. I was measuring deflections in the order of 4 or 5 in., so, therefore, I am able to remain with the linear theory.

MR. FRANDSEN: Would you say again how you extracted the M components, the moments from your information about θ? Would you go through, again, how you get the dipole moment?

MR. TOSSMAN: Yes. In the paper I have the exact equations, the exact response and how I get that constant of proportionality between the response slope and the dipole moment. I didn't go into all the mathematics here. I don't want to go through all the equations. We just have a simple string mass system. Here, we

are using a torsional system with the moment of i tia, the acceleration plus spring rate, deflection response, and this is equal to torque because of the MH. This being one component of dipole moment, this being the square wave magnetic field. So, this expression is driving the spring mass system at resonance. The deflection at response will be of the form of t sin ωt, a linear increase in amplitude with time, and that the slope of the increase in time is proportional to the dipole moment. I am using θ_n to mark the position of the deflection response at the half-cycle point. Because this is actually what I measured, this is equal to 2H. This being that component of field I am generating, and this being the value of dipole moment, which is orthogonal to the field divided by the resonance squared, I. In other words, I am generating my field in this direction. And the dipole moment here is pulsing the body into resonance. I measured the period, I measure precisely the moment of inertia. You can determine precisely the field that is being used.

Plotting the deflection response, measuring the slope, then gives an accurate measure of this component of the dipole moment. Then taking off this field, generating one – say, in an east-west direction – you can measure this component of dipole moment in the body. Because the body rotation is very small, you just add these two vectorially, and that resolves the dipole moment of the center.

MR. BROOKS: Just for comparison, you already said it was something on the order of 50 pole-cm. Do you know offhand what this would be in gamma at 3 ft?

MR. TOSSMAN: You can figure it out just by assuming – taking 50 pole-cm and assuming that to be a pure dipole moment you can figure the field at 3 ft. I am not measuring field, I am measuring the torque response (which is a little different). It is unusual and it is promising.

MR. BROOKS: The reason I asked is that it would be an alternate way of getting the moment.

MR. TOSSMAN: Definitely.

CHAIRMAN GAUGLER: Is the pole-cm the same as the cgs units?

MR. TOSSMAN: Yes.

CHAIRMAN GAUGLER: Mr. Parsons, wasn't 25 cgs units 0.25 gamma, or something like that?

MR. PARSONS: It is in there. I think 150 would be about 1 gamma.

MR. WOOLLEY: If you have a dipole that you can consider a point of 50 pole-cm strength at a point 100 cm away, you will have 10 gamma.

OPEN DISCUSSION ON PREDICTION AND ALLOCATION OF TOTAL SPACECRAFT MAGNETIC MOMENT

CHAIRMAN GAUGLER: Well, now we are going to try to take a real bear by the tail here. To start the ball rolling, I would like to call on Ken Goldstein of Texas Instruments.

MR. GOLDSTEIN: My partner is Frank Riley, also from Apparatus Division in Dallas. Some time ago I was told that working around high magnetic fields has a tendency to induce baldness. And I am not sure about high magnetic fields, but Frank and I are kind of living examples that there may be something to worry about in low fields also.

There was a question raised this morning: How do you resolve the vector dipole moments of several thousand components? As the speaker pointed out, it is a real bear. One possible approach was investigated — I should say a preliminary investigation was made at Texas Instruments under contract with JPL some time ago. The objectives of a statistical approach to the problem of allowable magnetic moments in a spacecraft are actually two-fold.

First of all, given the coordinate location of an electronics package that contains N dipoles, whose position orientation and magnitude are given by probability density functions, derive the probability density function of a component of the magnetic field at a sensor outside the package. Well, then, let's turn the problem around.

Given the allowable probability density functions of the components of the field intensity, it is desired to be assured, within some probability, that the absolute value of the field intensity will not exceed some prescribed value, and then determine in what manner the original parameter should be adjusted to meet this.

Well, the initial approach is, first of all, define yourself a coordinate system. After some investigation, we found that working in the spherical coordinate system has several distinct advantages when you start manipulating some rather involved statistical terms. To keep this a very brief and very general presentation, I will spare you some of the brutal statistics involved. This is documented in a report that was made to JPL.

From the equations for the magnetic field in spherical coordinates, we obtained equations for the X, Y, and Z components in terms of their spherical components. The purpose of this is to refer back to rectangular coordinates for compatability with the sensor or magnetometer coordinate system.

Now, a component of the magnetic field intensity — say, the component in the X direction may now be expressed as a function of four random variables. M, the magnitude of the magnetic moment, and using the parameters of the spherical coordinate system, ϕ, θ, and R. What we are going to do is derive probability density functions of the position, orientation, and magnitude of each dipole in the spherical coordinate system.

Basically, as a little review, a probability density function is obtained from a histogram or plot of the number of occurrences versus the value of the occurrence. The probability density function is obtained as the sampling interval of the histogram becomes infinitely small and the number of observations becomes quite large.

After obtaining the joint probability, the joint probability that we have now, which is a joint probability density function in terms again of M, R, ϕ, and θ, the problem becomes to translate this or transform this into a probability density function for H_x, or the x component of the field intensity.

The first step is to directly transform the joint probability density function to a new joint probability density function of four new variables, one of which is H. Then, obtain the density function of H in the x direction by integrating the new joint function over the limits of the three extraneous variables. I won't go into the exact manner in which this is done.

Now, we have to have assumed probability density functions of the random variables. We must have the functions of M, R, ϕ, and θ to evaluate the probability density functions of H_x, H_y, and H_z. Preferably, these assumptions should be made only after considering histograms of empirical data from a fairly extensive components measurement program. This is a big point that I am going to repeat in a minute: out of a statistical study, one gets only the quality of the input data.

We ran through one example for a spherical package. The initial calculation was made for just one dipole in the entire population. We had to make some assumptions here. By way of reference, ϕ_1, θ_1, and R_1 are the coordinates that translate the dipole with respect to the center of the electronics package. ϕ_2, θ_2, and R_2 are the coordinates that rotate the axis of the dipole with respect to the coordinate system. ϕ_0, θ_0, and R_0 are coordinates that relate the center of the electronics box to the sensor. For the pusposes of this example, we defined the probability density functions and the assumptions are this: That all dipoles are uniformly distributed within a spherical package of radius R, and that the effective alignment of all dipoles

is uniformly distributed from 0 to 60 deg with respect to the z-axis with a probability density function for the amplitude of the magnetic moment. That is the probability density function assumed for R, the radius, for ϕ_1, for θ_1, for ϕ_2, and for θ_2. It is now necessary to obtain the overall probability density functions for ϕ and θ. They are obtained by convolution — the probability density function of θ is obtained by the convolution of the density functions for θ_1, and θ_2.

Again, for the simple case where we assumed uniform distribution. The density function for ϕ is obtained also by the convolution of ϕ_1 and ϕ_2.

The general solution obtained in an analysis like this is in the form of an improper integral of the second kind, which, in general, is not defined. It could be defined by the use of contour integration times the Jacobian of the transformation.

Because the probability of H_x has even distribution around the origin, a mean of zero in unit area, we can equate the probability of H_x to one and solve for the range — some range of H_x. This very lucid description is for just one dipole. The question now is how do we extend this to N dipoles.

Well, when N is a very large number, the probability density function of H_x tends to approach a gaussian shape.

For two dipoles, which is the probability of H_x convolved with itself, and for three dipoles it is beginning to approach at least a gaussian shape. So, on this basis, when N becomes a large number, we do have a gaussian curve that is characterized by its mean and its variance.

So, the curve for the effect of N dipoles will be N times the mean value of the probability density function for one dipole, and the variance of the curve will be N times the variance of the probability density function for one dipole.

We also want to turn the problem around, as I initially mentioned, and the determination of component parameters from the model — given the model of the gaussian probability density function in terms of H_x and N, having in this case zero mean, centered about the origin — pick a range of permissible values of field intensity, H, in this axis and choose some probability that a sample value of H chosen at random from the total population will fall within the required limits.

In this case, the area lying between plus and minus H on the curve represents the probability of occurrence of probabilities within those limits.

By going to tables of normal distribution, you can come up with a Z number. For example, for the three signal limits I think it is 2.73. On this basis, you can define the variance that is in terms of X and N, the sample, the total number of dipoles equals H^2/Z. H is the limiting value that you originally selected. What you

have to do here is go back to the equation for the curve, and calculate the variance, and then compare that with what your requirements say and play the parameters in order to get what you feel you require.

There are other physical configurations that could be discussed. One which is especially interesting is a sphere truncated by two planes parallel to the X-Y plane. This begins to bear some resemblance to the main electronics bus on Mariner IV. It is an approximation to that. It is now possible to modify the original probability density function of ϕ_1 and R to be written in terms of the compound probability that you introduced by limiting the figure from the sphere to a truncated sphere.

It has also been mentioned today, by several speakers, that there is a relatively severe problem of magnetics stability that we know fairly little about. If you consider a magnetic unit, such as a transformer, the magnitude of whose magnetic moment decays with time in a probabilistic rather than a deterministic manner, it is possible to write the magnetic moment in a probabilistic form. And the problem then evolves into determining a probability density function for the given component of field intensity in terms of this new parameter.

Well, I'll sum this thing up with a few comments. The statistical approach — and this is just a first trial — tends to free the designer from making a precise field calculation for each component and each possible configuration. But, this is achieved only at the expense of accepting statistical rather than precise answers.

I would also like to point out that this derivation is just a first step in a complete analysis of the problem. The success of the mathematical model depends very critically on the inputs to the computation and on the definition of the probability density functions. What needs to be done to determine these is to conduct a component measurement program, plot some histograms, so that you can (with some degree of confidence) then determine probability density functions of the random variables from your test data.

This type of an approach may be a fairly useful tool to define realistic magnetic specifications for spacecraft in the future.

MR. PEIZIER: I noticed the agenda mentioned the prediction of fields, and I haven't heard much except this one speaker on trying to predict the fields of items before they were measured. I was just wondering whether any work was done in this way; that is, given an object that's going to be such-and-such dimensions what's the field?

MR. IUFER: The major difficulty in predicting fields is in the oversimplification of assumptions. We have looked at various ways of predicting fields, and tested these ways by looking at the individual magnetic properties of parts, and measuring them as they go into modular construction, and measuring them when they are on the mother boards, and measuring the mother boards when they are in a stack, and then stacking the experiments on the shelf and then measuring again. The conclusion of this effort, essentially, has touted us away from statistical methods or analytical methods. It seems that Maxwell probably knows more about magnetics than most of us, but probably he couldn't make a clean spacecraft.

So, there seems to be something else that, at least at the start of Pioneer, was definitely lacking; specific empirical information on the actual materials that you are now building spacecraft from. Basically, how we are proceeding now is to measure the magnetic moments of individual parts like a transistor with its kovar leads clipped to 1/8 in. or 1/16 in. After we have individual parts measured, we sum these up.

Then we pay attention to the orientation because parts may have a ratio of magnetization of maybe 5:1 with respect to its axes. We found also that, in most cases, the rule of linear superposition applies, and this was a great help to us.

On Pioneer, we have something like 10,000 or 12,000 transistors, and we tried to distribute these so that no one axis was favored in the initial design, and we subsequently modified this to favor the B parallel to the spin axis. As it stands now, you measure your parts and add them up. That's the field of the whole spacecraft. The multiple effect is not significant when you normalize the overall magnetic moment by a single axis exposure. The general criteria for what is good for space is not too well defined. What you would like to do is make a design that you think will achieve the objectives of the mission, and then perform a test that can be reproduced, you can pay money on for an incentive, you can reject the box on it if it fails — this has to be airtight.

Some of the numbers we found — we feel that we can measure the remanence of an experiment to approximately 5% without using elaborate techniques. We can measure the three-axis components of the remnant moment in an experiment in less than 1 hr, data reduction included. And the instruments that we use for data reduction consist of a 10-in. slide rule. The error that we got when we summed up the fields for six experiments in Pioneer was approximately plus or minus 10% over the predicted values when you added up the measurements of the boxes, separately.

So, does this answer your question?

MR. PEIZIER: Well, I think it does in a way. Perhaps measurements are much less of a problem for you than it would be on a mine sweeper.

MR. IUFER: Yes, it took us 2-1/2 days to measure a mine sweeper, and we can measure an instrument now in 1 hour. We can measure a spacecraft in about 1 day.

MR. PEIZIER: It takes more than 2-1/2 days to measure engines from the factories.

MR. PARSONS: What Mr. Peizier had in mind when he spoke his first words was to theoretically compute the field from the whole assembly or from one piece without any knowledge of the hardware other than the permeability, shape factor, and a few other things, which, so far, I have been unable to do on anything approaching a complete set of hardware.

However, we do have a set of data comparable to what Mr. Iufer was just speaking of. I have a couple of numbers here I will mention. We computed in one case that we would expect to see 22, in another 22.7, for a post-exposure field because of all parts aboard. We then measured all the facets individually. In one case we got 22, and in the other we got 18.

MR. CHRISTY: We are someplace between the Pioneer and the mine sweeper. Instead of 120 lb on the Pioneer, we are in the 500-lb category with the Mariner class spacecraft. We are also talking now of some 36,000 individual entities per spacecraft and the problems become a little more involved. We had initially considered, from the Mariner II experience, linear superposition of fields as you use. We ran into no end of headaches.

Even considering the subassemblies, as the smallest unit that we looked at, where measured fields for a total spacecraft were on the order of 30 or 40 gamma, superposition brought us up over 200 gamma at the sensor.

It was significantly above the measured fields. Now, we have taken the approach — that until the study from Texas Instruments was done for us — that I guess was really suggested by Professor Leverett Davis, Jr. from CIT. This was one that reflected basically the experimenter's interest, and that's namely the total field at the sensor. Granted, this is not information that's extremely worthwhile, but the experimenter is interested in how much of a field he has to fly in, and how much saturation he has and this sort of thing. Does the field come anywhere near his dynamic limits such as this. So, on this basis, Professor Davis suggested

that we group the fields as contributed by these various subassemblies in order of magnitude categories, and then you take the highest field and consider this as your prime contributor, and largely on the basis of experience and knowledge of the package, you sort of adjust this as extrapolated to the sensor location.

You sort of adjust this: wobble about the point where it, oddly enough (after-I guess it was after the second Mariner Venus spacecraft) we did a little better job of predicting the fields than we have done with slightly more scientific (shall we say) methods on the Mariner Mars class. Granted, this is witchcraft, and we hopefully have gotten away from this. This was one reason why the statistical approach is very promising to us.

MR. IUFER: Maybe we ought to point out now that what can be used in one program isn't necessarily true in another. We found that the rule of superposition worked very well for the experiments on the Pioneer because there were no permanent magnet sources, and if you placed the magnetometer right on the skin of the experiment, you could measure a field of only a few 100 gamma that, when decayed by some few centimeters (to the next box), did not produce an induction field of any consequence.

If you had a permanent magnet aboard for some device, it may have an immediate field, or, I should say, a field in its immediate vicinity, which was high enough to cross-talk or produce an induction moment in a nearby box. It has also been our experience that, when you are trying for an extremely clean spacecraft and you have good information on a rather restricted number of alloys, you are now able to predict what fields will be. We can trace essentially all of the magnetic material in our experiments to Kovar, which we could not eliminate because it is the only material acceptable for hermetic glass seals.

We have sort of reached the point of no return, to go much further you would have to develop a new method of sealing. In other programs, you may have motors and all sorts of equipment representing alloys of all sorts of different saturation characteristics, permeabilities, temperature coefficients of permeability, and ability to have remembrance.

CHAIRMAN GAUGLER: Are you suggesting that with a clean spacecraft you tend to get this linear superposition?

MR. IUFER: Yes.

CHAIRMAN GAUGLER: Well, there is another advantage in making a clean spacecraft.

MR. CHRISTY: If you had a nice clean spacecraft, you wouldn't have to worry about that.

DEVELOPMENT OF MAGNETIC TEST FACILITIES AT GODDARD SPACE FLIGHT CENTER

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Before you is one of the novices that Mr. Parsons referred to yesterday. I have been interested in magnetics only for the last 4 years, and I'm going to try to compress some of that experience into the next 20 or 30 minutes.

The qualifications on which I was selected to develop these facilities are more or less unknown to me. I can draw on great abundance of ignorance in magnetics and perhaps this means that I am not quite sure where we have to stop. The other quality that I think is essential to any of these enterprises; when I am speaking subjectively of myself I like to call it tenacity. I hear some of my project engineers muttering about unreasonable and stubborn and a few other well chosen endearments, but in many respects this is one of the keys to developing a successfully magnetic test facility. We run a constant battle with our facilities people on the selection of materials. We started out by specifing permeabilities to be less than 1.02 and these to be distributed. We thought this was pretty safe because there is quite a gap between the ferromagnetic materials and the nonmagnetic materials.

We found our facilities people to be extremely clever at finding things right on the border line, especially things that look well at about 1.02 or less, in an as received condition, but as soon as they are stressed or work hardened they immediately jump up to a higher permeability. So we have had to fight with them constantly about the absence of reinforcement in our concrete and means of making large monolithic structures strong enough and stable enough to support the coils. Yesterday, Mr. Casani spoke of one of the reliability criteria being adequate ground test ability and Mr. Maclay spoke of including, amongst the design restraints, the means for contractors to meet these restraints. This is pretty much the purpose of what we have been doing with these test facilities. We feel that a certain number of tools are required to carry out the test program. Some of these can be classed as essential, you couldn't very well do this kind of testing without at least a magnetometer and a reasonably clean magnetic area. Others are more in the nature of the desired tools that will contribute to the speed, safety, and improved accuracy of testing. And a third level, if you wish, would be considered as refinements or convenience features. These are the ones that come into the cost of the thing and are usable for negotiation

purposes. They are the things you can do without, you would like to have them if you can get them cheap enough. Naturally, the choice of facilities required depends, to a large extent, on the mission. If the mission involves the repeated testing of large numbers of very small components, very small facilities are required. Flux tanks are usable in many cases and, in general, requirements for large homogeneous volumes at zero field do not exist.

On the other hand, if the testing involves development of magnetometry, this type of facility does require reasonably large homogeneous volumes in which the largest instruments expected can be totally emerged in homogeneous field. Another essential feature of the facilities was brought out at great length yesterday, and that is the stability. Earth's field unfortunately has the characteristic of continually changing. There are periods running up to several hours when it is relatively quiet. These are not readily forecastable. In many cases, the diurnal variations are not going to be sufficient to jeopardize the validity of a test.

In 1961, the Goddard Space Flight Center started seriously examining means to meet needs that started evolving back in 1959 and even earlier, and it has taken the last 3 yr to bring an effective facility on line.

I am going to take you for sort of a travelog through our facilities to show you what we have. Some of you may be building or contemplating building facilities. We'd like to share our experience with you and help you in any way we can. To repeat what I said earlier, the needs really must be assessed on a case by case basis, largely depending on the mission to define what are the things that are really essential, and what are the things that are desired, that can be justifiably amortized over a period of a few months or a few missions. At some later date when you get into the actual specifications of test equipment, then you start to look at refinements and sophistication that may make good negotiating tools or that would be desirable, but not really necessary, items. Some of the first answers to determining what is essential and what is desired can be derived from examining what is available and where it is. One of our problems at Goddard has been one of logistics. We have available to us the magnetic observatory at Fredricksburg, which is an excellent facility, but it is a 1/2 day drive. We have had crews of men working there for some time, working late at night just to try to complete their job and there is about an 8 hr turnaround just in the travel. If you want to run down and run a test as you may in a development program, this becomes a bit of a nuisance. It is also necessary to get some idea what the existing work loads may be in existing facilities. In other words, you must ask the question, can I get into the facility even if it's one

that I can use. We have been very fortunate in having the cooperation of the Coast and Geodetic Survey people in allowing us the use of Fredricksburg, and I think we would have been completely lost if we had not had the cooperation of Jim Ford's group at the Naval Ordnance Laboratory to help carry us over a period when we weren't equipped.

Assuming, then, that you know what you need and know what you want, the program is going ahead. You first have to examine some of the site criteria. Again the question of logistics; how far away from your base of operations can you place this, or how near can you get, the availability of power and water and sufficient space to provide the isolation required from artificial anomalies. The next step would be the generation of facility specifications. It is quite possible, if you are answering solely a need to combat a logistics problem, that perhaps an established system or established facility (that may be something essentially off-the-shelf) will be satisfactory. It is also desirable, of course, to try to forecast a little and find out what the current and future needs are.

With these few words of introduction we will now discuss the pictures of the facilities. I will go through the facility in about the same order that I have taken on numerous occasions when inspecting the construction progress. I will try to give you a few words on each of the facilities and buildings in the photographs.

The map of Fig. 1 doesn't show the outline of the facility site, but we have about 125 acres located approximately 1 mi due east of Goddard Center. Our nearest known problem in terms of anomalies will be the launch phase simulator presently in construction, which is essentially a 60-foot arm centrifuge, and will be about the nearest point to the magnetic facility on the main Center.

We come in through Telegraph Road, and I'll stop first and show you the exterior of the Laboratory Control and Monitor Building. This is a conventionally-constructed building. It houses the distribution of services and our mechanical plant. The Operations and Instrumentation building provides space for the control consoles associated with our Magnetic Instrument Test Lab, which is just about to come on line, and for the Attitude Control Test Lab, which will come on line in 1966.

We'll go, then, down to Magnetic Quiet Building No. 2, which houses a 15-ft square coil on a wood frame, used largely for preliminary development work. And back up to our Component Test Lab, which has been in service since 1962. It was built in haste on emergency funding and was put up as a temporary building. It is not particularly magnetically-clean; in fact, we are working now to change the name from the temporary lab to the component test lab.

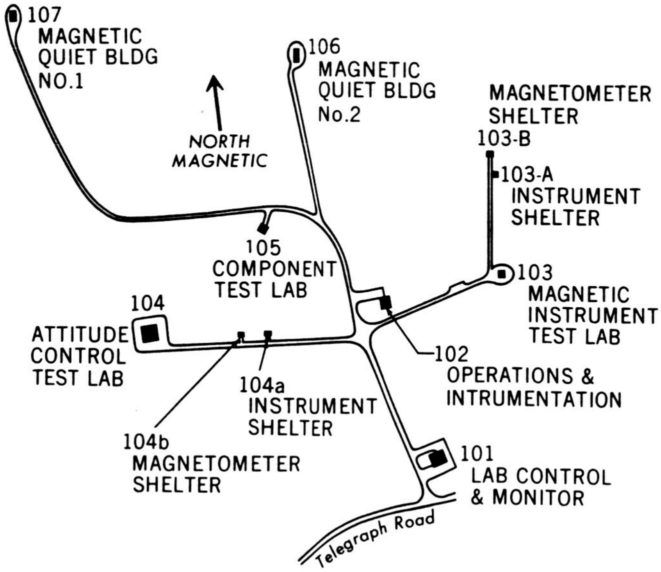

Fig. 1. Magnetic test site at Goddard Space Flight Center

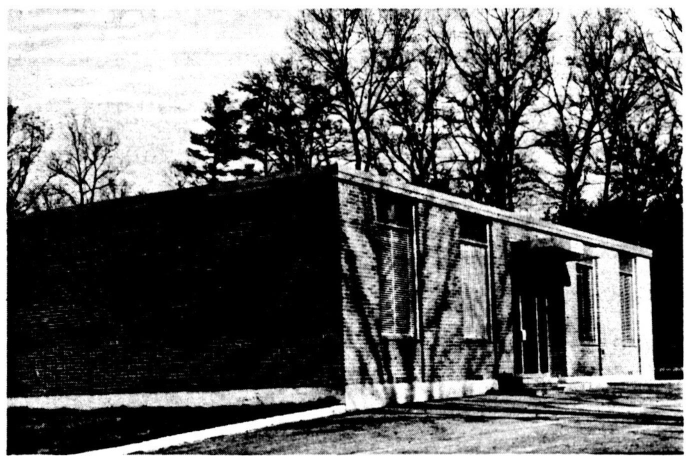

Fig. 2. Laboratory Control and Monitor Building

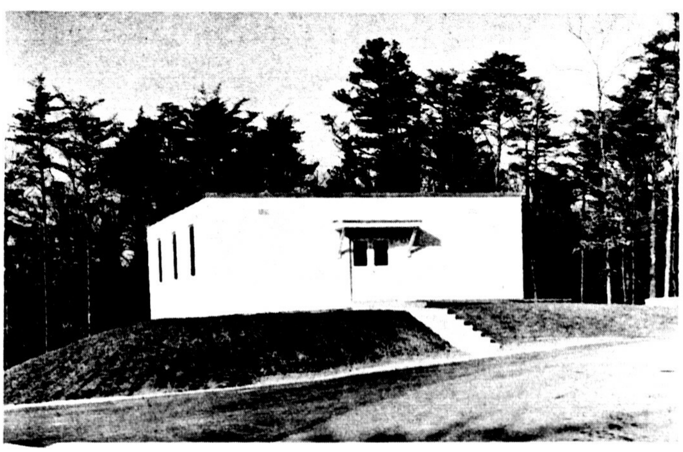

Fig. 3. Operations and Instrumentation Building

And we go on out to the Magnetic Quiet Building No. 1.

Figure 2 shows the Laboratory Control and Monitor Building. There are about 5000 square feet of space. There is a basement underneath the building, a large laboratory on one end, a medium-size lab on one side, and administrative offices in the back for the magnetic test section. This is all conventional construction.

Figure 3 is the Operations and Instrumentation Station. It's split down the middle, dividing it into the two rooms, with the control room for our magnetic fields component test facility or Magnetic Instrument Test Lab, which is to the east, on the east side with windows that will provide visual contact with the laboratory building. On the west side, through the windows, we have visual contact with the Attitude Control Test Laboratory. There is also a basement under this building for relatively clean tests, but not those that are most demanding. This building itself is clean, with the exception of a few bimetal elements and some valve cores that could be replaced, and some power switch gear that could be replaced, if at some time we need to make this magnetically-clean.

Figure 4 is the exterior of our Magnetic Instrument Test Laboratory, commonly known to us as "the 20-foot coil building." It is entirely frame. We have put hatches in the sides on the coil axes in case we have long booms that must protrude out of the facility. It's a pretty close fit. We have a 22-foot coil in this building, which is 30 by 30 by 30 ft.

Figure 5 is a model that was made by the contractor who built our coil. It's a four-loop system. It is actually a Braunbek configuration. With the governing criteria we figured, using the Braunbek spacing, we required a 5-ft square access opening in two sides and the bottom.

Figure 6 is one of the problems that we encountered. I am not certain how readily apparent it is, but in making this Braunbek coil, it was determined by computer studies that we would have to hold tolerances in the order of 0.007 in 20 ft. This was a little bit ridiculous. We settled for 1/32 in. in 20 ft. The coil itself went together with hand fitting. One of the means of achieving the precision of locating the coils was to use aluminum strip winding rather than conventional round wire. This sounded like a real good idea at first, and we explored the possibility of anodizing the aluminum strip and finally came up with the idea that one of the varnishes applied on the outside was a more reliable finish. The anodizing seems to be variable, depending on the process and who does it, and we found, in some instances, that it is very hard and, in other instances, that it is very soft. So, we got wire

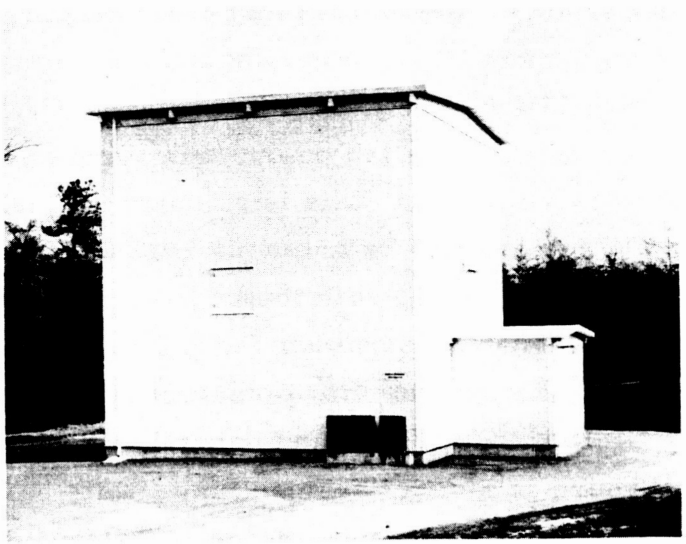

Fig. 4. Outside of Magnetic Instrument Test Building

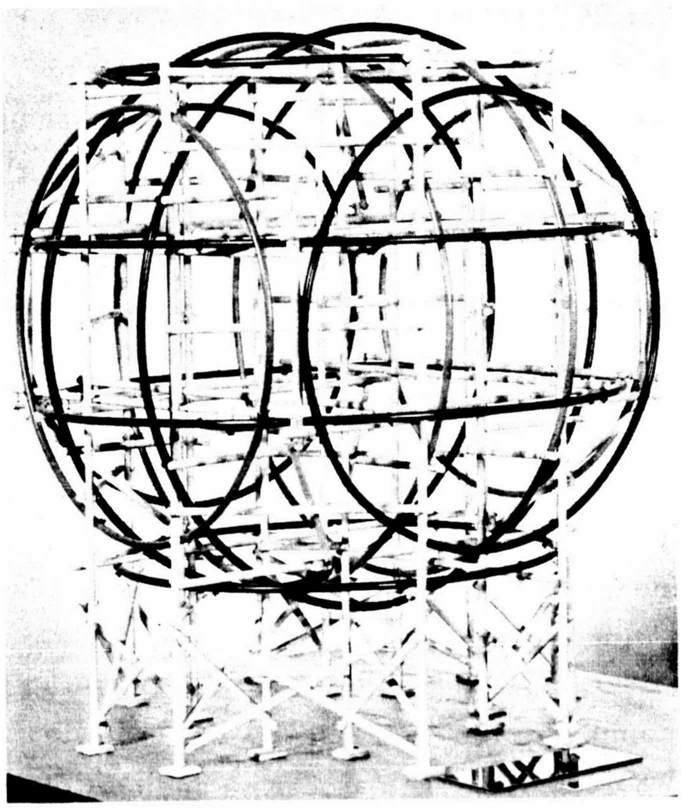

Fig. 5. Braunbek four-loop coil system

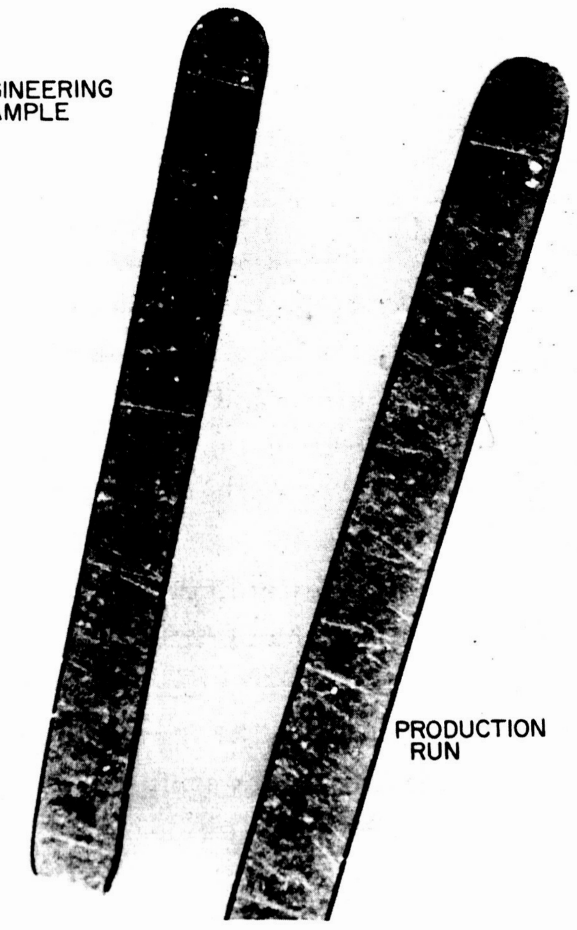

Fig. 6. Cross-section of aluminum strip coil winding

with insulation applied on it. It looked very fine. We wound a test coil — our contractor did, that is — and it came out very well.

And, when we got the production run of wire, we found we had a wetting problem at the corners. We got a little cap of insulation out on the end, a gap, and then insulation on the sides. So right from the first of our coil-winding experience, we started shorting out turns and finally had to go to placing a wrap of Mylar partially around the turns, from the top, over the edge, down the bottom, and up over the other edge. This worked out quite well. Our coils are all perfect, and there are no shorts in the structure.

Figure 7 is the construction photograph looking north inside the building. There will eventually be a work platform, at the door level. You will notice that we prewound all of the coils; we set up a merry-go-round on the floor, machine-wound the coils, and laid them up against the appropriate wall or the ceiling, as the case may be, and then interwove the structure with the coils in what seemed to be an almost impossible task.

As a matter of fact, they did have to cut holes in our ceiling to preserve some of the long columns that they had specially selected, rather than cut and join the columns.

Figure 8 is an interior view of the completed coil. In the center is our test fixture on which we made acceptance tests of the homogeneity at the center of the coil. Specification called for 0.001% homogeniety over a 3-ft sphere at the center of the coil. Without the use of gradient canceling coils, I think we came to just under 1 gamma in this 3-ft cube. With a small amount of gradient canceling, we came down to 0.001% or less than 1/2 gamma. It was more like 0.2 to 0.3 gamma throughout this cube at the center of the coil. The measurements were made by a rather tedious process of taking a single-axis magnetometer, placing it in the center, checking zero field, flipping it, checking the magnetometer, and then moving it out to an end station, and back to the center. We went from the center out in all directions and got readings on the nine partial derivatives of the first order gradients in the coil.

Figure 9 shows the shelters that we put up for our station magnetometer. The magnetometer will go in the rear building instrument shelter housing the servo-system controlled by the rubidium vapor magnetometer. These are located on the magnetic meridian about 350 ft north of the 20-ft coil that we have just seen.

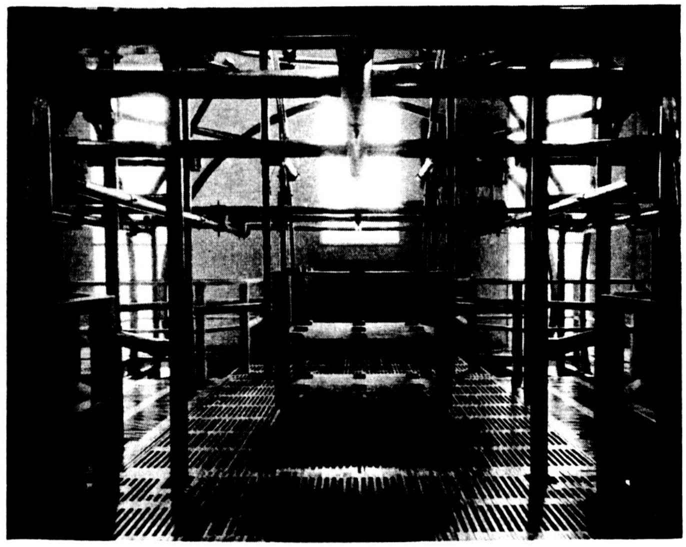

Fig. 8. Inside of Magnetic Instrument Test Building showing completed coil

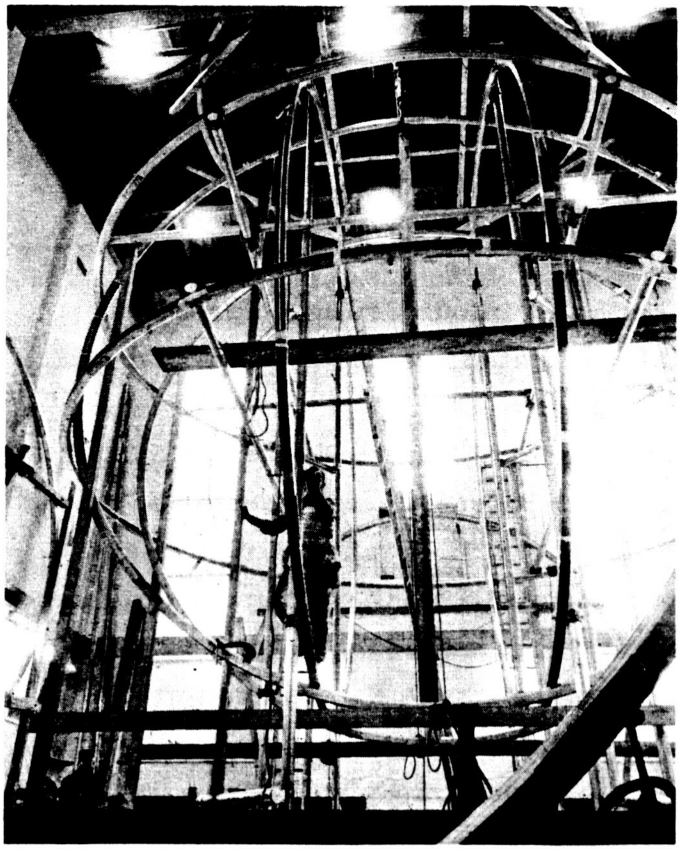

Fig. 7. Inside of Magnetic Instrument Test Building during construction

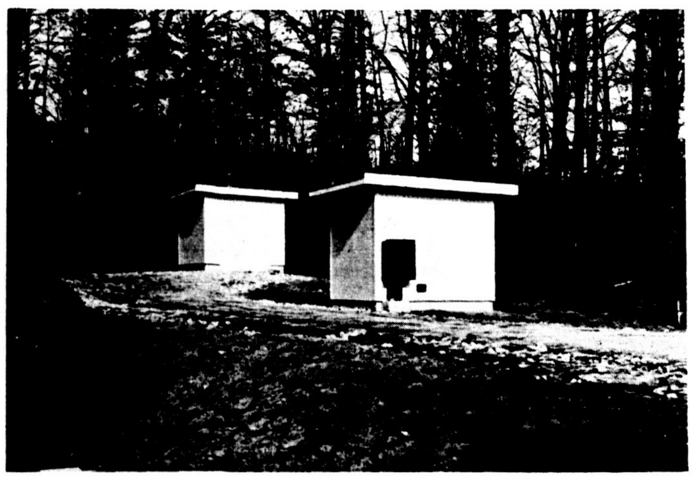

Fig. 9. Shelters for station magnetometer

Figure 10 is our Attitude Control Test Laboratory. It is 60 by 60 ft in plan, and has a 50-ft high interior clear dimension. There is about a 17-ft base, and it is about 35 ft above the ground.

That's a real barn when you look in it. None of us envy the acrobats that are going to have to put up the coil system in there. I haven't provided any pictures of this building, because there isn't anything in there; but the equipment is under contract. This was all made of selected concrete masonry units. They stacked up the concrete blocks out in the parking lot area, and we inspected every one with a gradiometer for magnetic cleanliness and rejected those that did not appear to be satisfactory.

Figure 11 is our Quiet Lab No. 2. It is a 16 by 50-ft building, open on the inside. It is isolated. There are services to it. There are telephone lines and power lines, but no water or air conditioning. Heating is from electrical panels in the ceiling. It has been used, as a matter of fact, for magnetometer development work.

Figure 12 shows Mr. Parsons in the center of the 15-ft square coil system. It runs all the way around the floor. This is a four-loop coil on the H and Z axes and two loops on the D axis, to provide the widest possible opening for access.

Figure 13 is a view looking south on the interior of the same building. We have a work platform that will probably be used more as a work bench on the east wall, and there is electronic equipment down at the end of the room.

Currently, under the platform, they have installed a Roots blower and Wells pump for pulling down a vacuum in a bell jar that they can mount in the coil for making heat-balance measurements on functional magnetometers.

Figure 14 is a Component Test Lab, which was built in great haste in 1961 or 1962, located at an angle to the magnetic meridian. It is not a very clean building, magnetically, there are quite a number of pins and small hardware used in the assembly of the structure that are steel or conventional building materials. I thought Mr. Parsons was going to have some pictures of the interior of this one yesterday, so I didn't bring any.

We have inside this a 14-ft square Helmholtz system that we got on surplus from the Navy Weapons Plant, and it has been a real work horse. It is going every day most of the day.

Figure 15 is our Magnetic Quiet Laboratory No. 1., Nos. 1 and 2 went back historically in terms of who got priority, and it shifted back and forth, but the

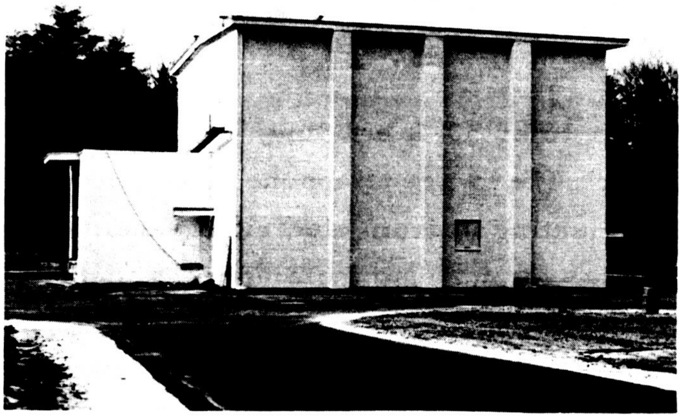

Fig. 10 Attitude Control Test Laboratory

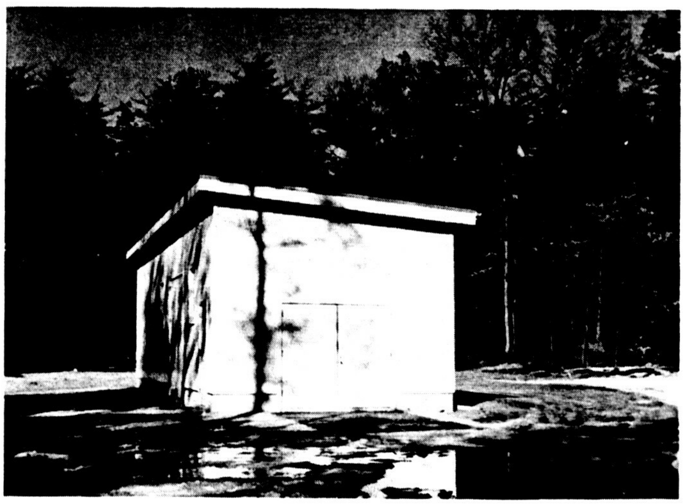

Fig. 11. Quiet Lab. No. 2

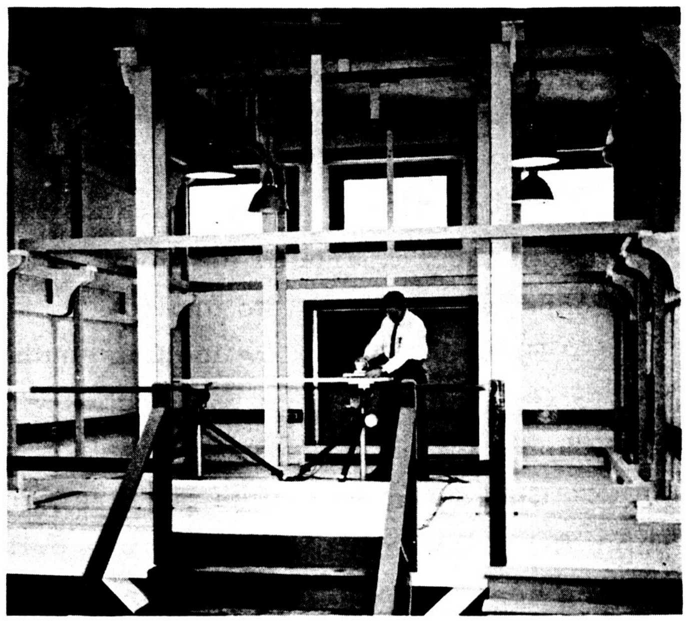

Fig. 12. 15-ft square coil system Quiet Lab. No. 2

Fig. 14. Component Test Lab

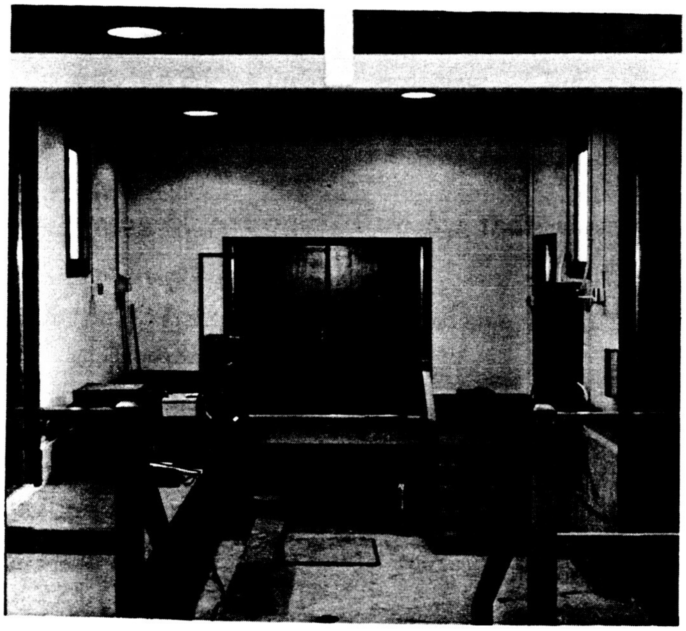

Fig. 13. Inside of Quiet Lab No. 2
looking south

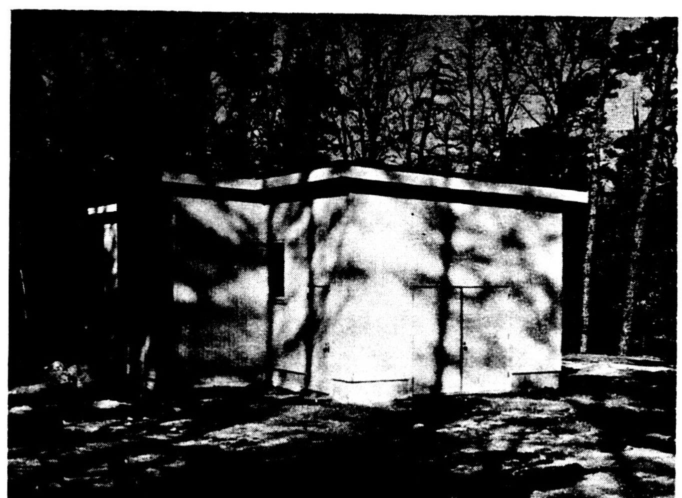

Fig. 15. Quiet Lab No. 1

numbers stuck. This building was originally intended to be identical to Quiet Lab No. 2, 16 by 50 ft, but then OGO got in the act and they decided they wanted the solar panels attached when they ran the tests, so we had to have vestibules on the sides.

After this picture, we have also had to add a vestibule on the south wall to enclose the AC magnetometer on the end of the boom.

Figure 16 is another view of the coil that we had in Quiet Lab No. 1. Mr. Parsons showed you some pictures yesterday of the OGO mounted in the building; this is a rather good picture showing the coil. Again, this one has a four-loop configuration on H and Z, and a two-loop configuration on the D axis. The coil is aligned on the magnetic meridian. We will put a similar bell jar vacuum system in this coil as soon as our OGO B tests are through.

Figure 17 is the interior of the Component Test Facility. There is one of the Z coils there, and another one just outside the picture, I believe. Figure 18 is one of our degaussing coils with Mr. Chuck Harris in the center performing some one of our tests. I am not sure just which one it is.

Figure 19 is a picture of the control console that we built inhouse to go with the 14-ft coil.

We are just coming on line with our 20-ft coil system at the Magnetic Instrument Test Laboratory. So far it has met all our expectations and exceeded some. We are consistently demonstrating noise levels in the order of 0.2 gamma. We do have a capability in that coil of rotating the field in any plane through the center of the coil system. This is an aluminum structure but we have insisted that every joint, every construction joint and all the joints in the coil rings be insulated; and they are, which gives it a very good dynamic response. It does, however, tend to slightly impair the shielding that we would like to get from 60 cps noise from our power lines. The 12-ft coil that we just looked at in Fig. 16 was not made for dynamic work. It has continuous rings and it does give approximately 6 db better attenuation at 60 cps at the center. The 15-ft square coil is on a wooden frame and could be adapted to rotating the field, if we so desire, at some time in the future. The 40-ft coil system will also have the rotating field capability. One of our purposes for going into a rotating field is because it appeared to us that many of our satellites are spin stabilized or have rotating devices in them. The problems of developing large nonmagnetic vacuum systems seemed extravagant, so we decided that the easiest thing to move was the field; it has no windage, no friction, and we can simulate the relative motion of the satellite with the surrounding field. This will give us one of our

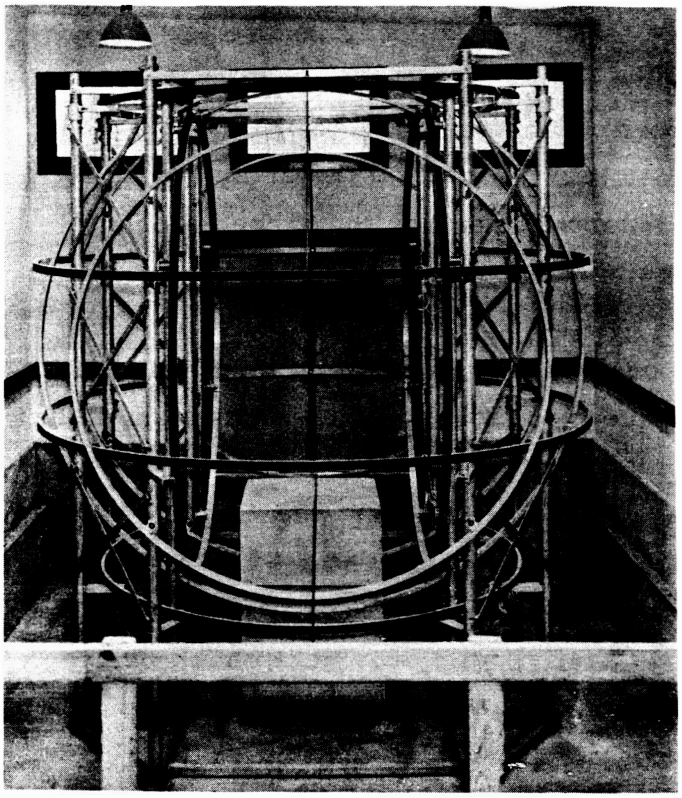

Fig. 16. 12-ft coil inside of Quiet Lab No. 1

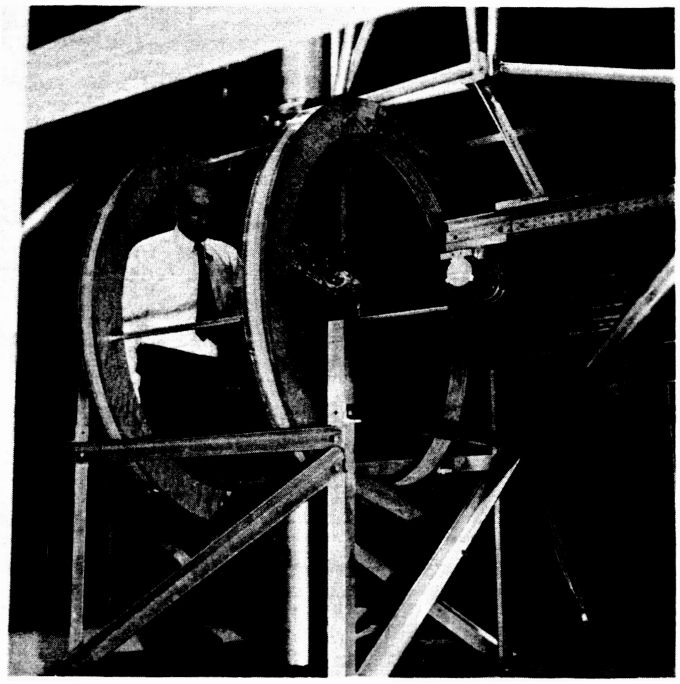

Fig. 18. Test Area in Component Test Facility Building

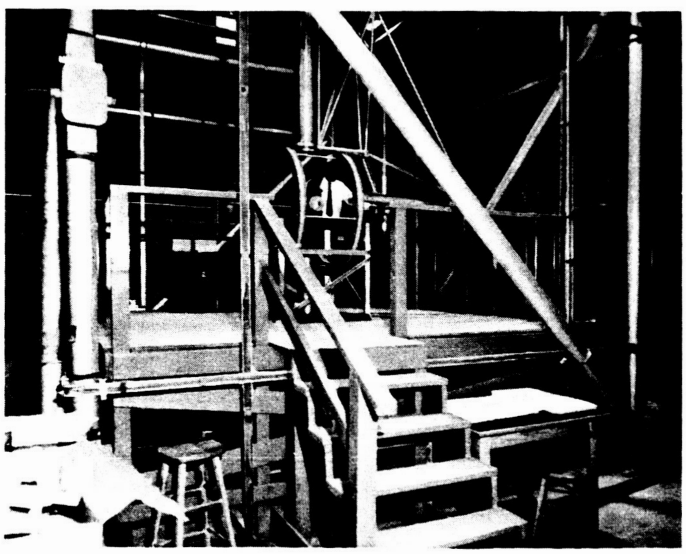

Fig. 17. Inside of Component Test Facility

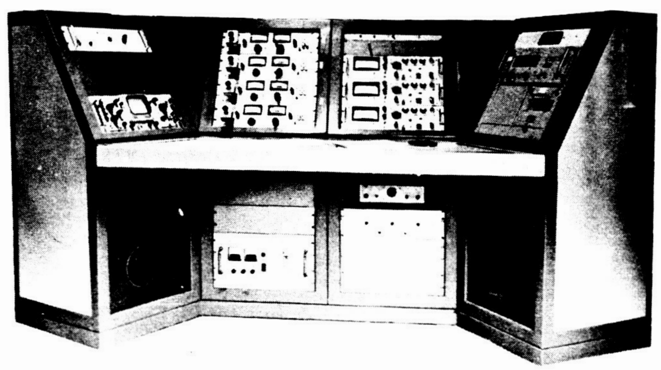

Fig. 19. Control console for 14-ft coil in Component Test Facility

main capabilities; that of being able to compute eddy current spin damping, actually measure the spin damping torques, and from a knowledge of the spacecraft dynamics work out the damping. Presumably to do a little better job of forecasting the need for on-board spin up fuel, we also plan to make some mechanical measurement of spacecraft static moments. This is collateral support equipment that is not yet under contract but will involve essentially a large float very similar to the system that Mr. Woolley described yesterday from the floor. A torsion wire or the three catenary chain system. We have no yet decided what we will use for a restoring force; but hopefully a means of positioning the spacecraft with some zenith angle other than that in which it is brought in. We have done some preliminary experiments with a model system of this kind and feel it is not out of the question to make such measurements with a resolution of about 20 dyne-cm. We don't yet know how big a package we can measure with this kind of resolution, but we are in the process of studying this problem. I am not sure this will ever prove to be a superior method of measuring magnetic moment to that of making the direct magnetic measurements, however, it is a back-up system that if it can be done reasonably easily, will provide a check on the magnetic data. In fact, in our model studies it has already detected one missed decimal point. We feel that it is already showing considerable promise.

There is one other piece of equipment scheduled to come for the 20-ft coil and that is a somewhat larger vacuum system. We are trying to enclose a spherical volume about 5 ft in diameter with vacuum to 10^{-5}, and hopefully thermal shrouds so we can conduct additional tests at zero field on functional magnetometers in this facility, to prove the thermal balance of the system. I think some of the pictures Mr. Parsons showed yesterday showed a large basketball sized enclosure around the magnatometer, these are usually striped or coated to control the emissivity of the surface, because the rubidium vapor magnetometers inside are somewhat sensitive to temperature. I am sorry that it was impossible to bring the facility with me other than in photographs, but I hope that we will have an opportunity to welcome all of you perhaps collectively, and certainly individually, at any time to inspect our facilities at Goddard.

OPEN DISCUSSION

MR. DROLL: Droll, from Ames Research Center. I am wondering, on your two quiet buildings, if you incorporated any new unique design features and, if you did, I am wondering if there is any advantage to working in one building over the other?

MR. BROWN: No, we did not incorporate any new design features in the quiet labs. We built them as cheaply and nonmagnetically as we could; and when that is nonmagnetic, that is exclusive of being cheap.

MR. MOSKOWITZ: Moskowitz, Rutgers University. How do you handle the car parking problem at the remote, sensitive, magnetic buildings?

MR. BROWN: I should have gone a little further into it on our site. We have placed the magnetic buildings 600 ft from the road, that is the Magnetic Instrument Test Laboratory. The Attitude Control Laboratory is 900 ft from the nearest point of the road. It is separated from the instrument test laboratory by 1400 ft. These two circles of isolation are tangent, and we do plan to park the cars down at the service building, the Laboratory Control and Test Monitor Building, the first one inside the gate.

To a large extent this may be contingent on the experimenter who is in there at the time. If he feels that his experiment is not sensitive to his car, he can drive it out there.

CHAIRMAN GAUGLER: Does your servo take out cars?

MR. BROWN: The servo won't take out cars.

MR. WEINBERGER: Weinberger, General Electric. You mentioned a three catenary chain system for measurement of dipole. Would you --

MR. BROWN: I don't want to steal Mr. Woolley's thunder. Is Mr. Woolley here?

MR. WOOLLEY: I can describe that system to you later during the break, if you are particularly interested. I am not at all sure, but what anybody planning to build such a device should give careful consideration to this system described to us by this John Hopkins fellow yesterday. It may beat mine by quite a ways.

ROOM-SIZE MAGNETIC SHIELD FACILITY

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Pasadena, California

OBJECTIVES

The primary objective of the room-size magnetic shield development was to provide a magnetic field environment located at (or very near) JPL, suitable for evaluation and calibration of sensitive space-flight magnetometers of both AC and DC type response. The main impetus to this development was provided by the Mariner Mars program, which included a new type of vector-measuring triaxial magnetometer of the helium type. The evaluation of this instrument required accurate determination of instrument-produced noise and zero-field reference offset. Another instrument with stringent requirements regarding a noise-free environment is the search-coil magnetometer such as that of the OGO program, which measures field fluctuations through 1 kc in frequency.

Secondary objectives were: to verify the validity of the shielding approach as demonstrated by the Socony Mobil shield located in Dallas, to obtain data for use in the design of future facilities, and to provide a noise-free facility for researchers in other fields requiring low magnetic field environments (such as super-conduction).

FUNCTIONAL REQUIREMENTS

To satisfy the objectives, the facility should be capable of producing a low field intensity of, say, less than 100 γ ($1\gamma = 10^{-5}$ gauss) with a noise level of less than 0.1γ and a gradient of less than 2γ/ft over a volume of at least 350 ft^3. Long term stability of the residual field should be on the order of $\pm 1\gamma$ over a period of hours. Because the facility was to be located in a region of man-made interference, a shield with its inherent passive noise suppression looked attractive. Furthermore, a shield would be more compact because it is capable of good field uniformity over its entire volume. As for long-term stability, the temperature environment needs to be taken into consideration, because unequal coefficients of expansion of the superstructure and shielding material give rise to strain, and most available high mu materials are strain sensitive.

LOCATION

For a minimum of noise interference and for low gradients, it seemed wise to locate the shield away from the laboratory complex proper. A location at the JPL Mesa Facility near the existing coil system was chosen because the shielding facility could be conveniently incorporated in the low-magnetic field facility complex. A disadvantage of this choice is the lack of an air conditioned building or enclosure at the location. Because the shield is at the mercy of the ambient temperature, the long-term stability of the residual field inside the shield suffers for the reasons mentioned above; however, a wooden enclosure has been built around the shield, and we hope to air condition the enclosure in the near future.

THEORETICAL APPROACH

The theory will not be developed here because it is not within the scope of this report, but a summary of approximate shielding formulas can be found in the bibliography. We will repeat those of fundamental importance, here, in developing the design criteria.

According to Patton and Fitch, the two-stage cubical DC shielding formula is approximately

$$S = \frac{2\mu_1 \ t_1}{\ell_1} \cdot \frac{2\mu_2 \ t_2}{\ell_2} \left[1 - \left(\frac{\ell_1}{\ell_2}\right)^3 \right] = S_1 \ S_2 \left[1 - \left(\frac{\ell_1}{\ell_2}\right)^3 \right] \qquad (1)$$

where

μ = the magnetic permeability

t = the thickness of the material

ℓ = the length of a side of the cubical shield

S = the ratio of H_o the external field, to H_i, the internal field.

The subscripts 1 and 2 refer to the inner and outer shields respectively. Patton and Fitch point out that μ_1 and μ_2 are here treated as independent variables instead of

constants as in previous shielding formulas. Therefore, the magnetic state of the shield must be known to select the appropriate value of μ.

The following formulas were derived for this purpose and are repeated here from Patton and Fitch*:

$$B_2 = \frac{H_o \ell_2}{2 t_2} \quad , \quad B_1 = \frac{H_o \ell_1}{4 t_1 S_2 \left[1 - \frac{\ell_1}{\ell_2}\right]^3} \tag{2}$$

where

B = the induction in the material

S_2 = the single stage shielding factor for the outer stage, computed from

$$S_2 = 2 \mu_2 t_2 / \ell_2 \tag{3}$$

The procedure is, then: calculate B_2; find μ_2 from the B, μ, H curve of the material at the operating point corresponding to B_2; calculate S_2; calculate B_1; find μ_1 from the B, μ, H curve at the point corresponding to B_1; and, finally, calculate S-the two stage shielding factor.

DESIGN

We decided to take maximum advantage of the experience of the Socony Mobil shield development, because the shield designed by Patton and Fitch seemed adequate for our purposes, and because we were working on a very limited time schedule regarding the Mariner Mars program. Reviewing the design with Patton on a consulting basis, we established the following important facts:

1. A shield of more than two stages is not justified for residual fields required to be no less than 30γ.
2. The higher the maximum permeability, the better.

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*B. J. Patton, J. L. Fitch, "Design of a Room-Size Magnetic Shield," J. Geophys. Res., Vol. 67, 1962, pp. 1117-1121

3. The thicker the material, the better.
4. The larger the space between shields, the better.
5. All machining and forming of the shielding material must be done before the annealing process is carried out (because of strain and shock sensitivity of the material).
6. The shield should be assembled on the superstructure on site so that no movement of the completed shield is required (for reasons given in item 5, above).

In addition to these considerations, it was deemed necessary: to handle all shielding material in a manner that would hold bending of the soft, annealed material to an absolute minimum and would prevent shock; and to make all joints between separate pieces so the average air gap is made as small as possible.

The material selected for shielding was based on ready availability consistent with the requirements for high permeability and a thickness at least comparable to the best crystal or grain size in the annealed state. 79% Ni-Moly Permalloy was found available from stock in 0.060 in. thickness, in widths to 23 in. at the Allegheny Ludium Corporation in the quantity required. This thickness is approximately the size of best grain-growth for this material. Calculations showed that use of this material in a shield of similar size and design to that of Patton's should result in a DC shielding factor of greater than 1000.

The final configuration is as follows:

$$\ell_2 = 10 \text{ ft}$$

$$t_2 = t_1 = 0.060 \text{ in.}$$

$$\ell_1 = 8 \text{ ft}$$

Calculations show a predicted residual field of the order of 10γ.

A box of 1 ft wall thickness was built for the superstructure. The box was covered and lined with 3/4 in. plywood, which acts as substrate for the Moly-Permalloy sheets. High quality, dimensionally stable lumber was used throughout. A layout was made for moly-permalloy plating and from this a parts list and a set of drawings for fabrication of the metal parts were generated. The metal was then

ordered prefabricated and annealed at the factory. The material was shipped, without transloading en route, by truck so handling could be supervised at both ends by qualified people.

CONSTRUCTION

The basic layout calls for a mosaic of 44 1/2 by 22 in. plates having 1/3 in. gaps between plates. Angles of bend radius greater than 1/4 in. cover the corners where sides of the cube join. Strips of Moly-Permalloy bonded to 3/4 in. plywood battens are laid over the joints to close the air gaps. A sketch of the shield is shown in Fig. 1.

This drawing is a little obsolete, I might mention; our electrical feedthrough openings wound up left of the doors. Instead of setting the room directly on a concrete slab, we have an aluminum I-beam base laid out. This was mainly to facilitate handling the bottom of the room during construction when we first had to cover the bottom of the shield and turn this panel over, put the metal on the top surface. It may not be very clear here.

The inner door covering the access door opening is a sliding door. This, we felt, would not interfere with working in internal volume as much, because it doesn't take up any room when it opens. The outer door is a simple hinged door. And you can see here, the batten strips laid along the joints are 1/3 in. gaps between the various pieces, and the angles I was speaking of are long angles of 3-in. legs and greater than 1/4 in. bend radius that go along all the edges. And, there are batten strips between the edges of these angles and the adjacent plates.

Figure 2 is a sketch of the treatment of the adjoining strips.

You can see here that this is the 3/4 in. plywood substrate, and we show here one of the 2 by 11 in. redwood joists or spacers that went in the wall to establish the spacing factor, and there are two of the plates with the gap, 1/3 in. between. There is a joining strip that has holes punched in it at 6-in. intervals and then these joining strips are bonded to the plywood batten. And the batten is undercut to distribute the load out near the edges of the joining strip for a very small air gap out at the edges.

There is an aluminum clamping washer, and this is also to apply the load at the edges of the batten strips. The assembly is merely clamped down on the surface by driving brass screws into the plywood.

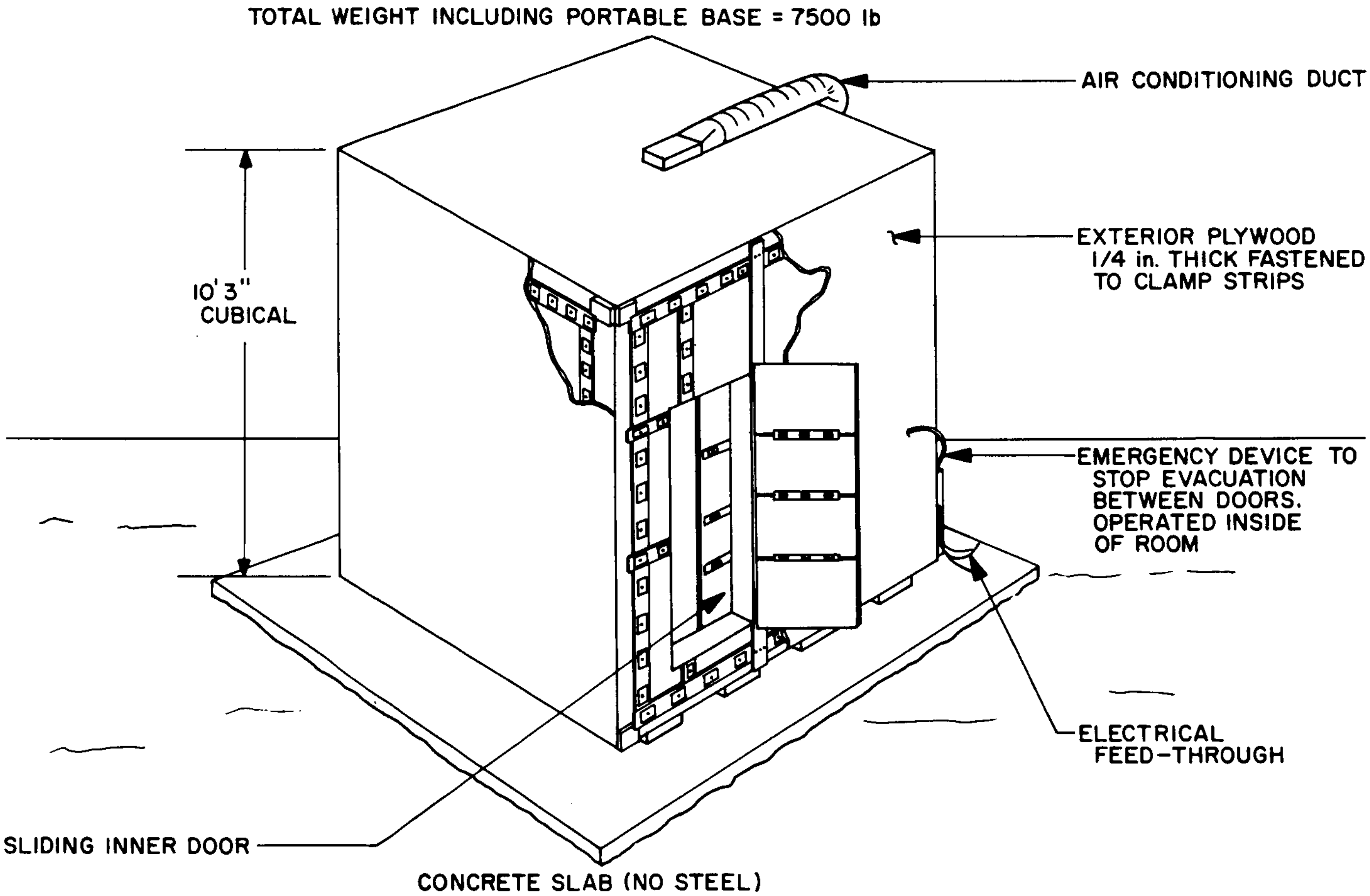


Fig. 1. Magnetic shield room installation at Mesa Site

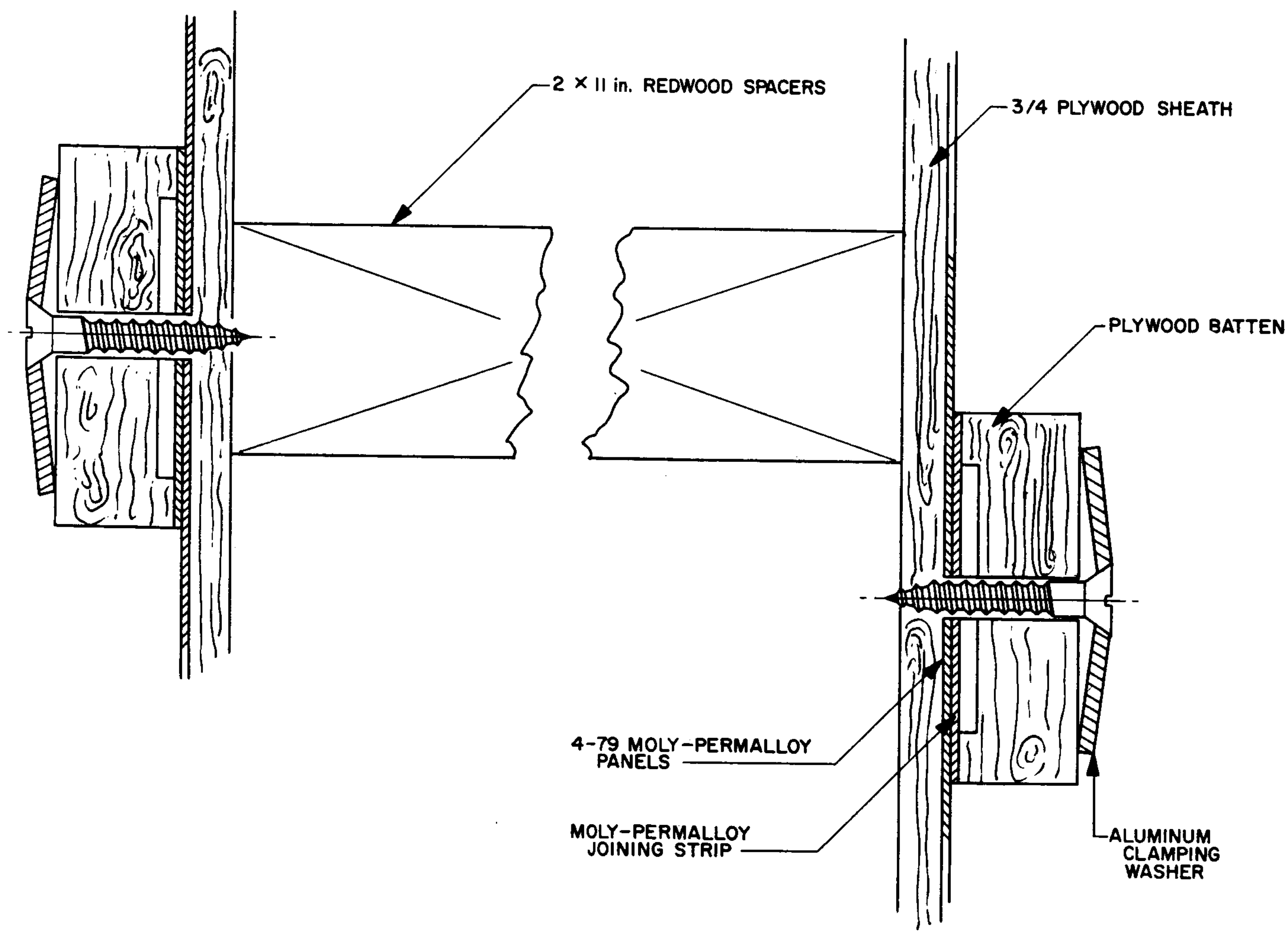


Fig. 2. Better detail of magnetic shield room

The inside of each wall, and also the outside, are both treated more or less identically in this respect.

Figure 3 is a sketch of the treatment of the floor, which I thought might be of a little interest. We have load-distributing plywood panels in between the batten strips that rest on the moly-permalloy plates on the inside of the room, and then there is a plywood sheet over this, which is supported by these load distributing blocks.

And, on the outside surface of the bottom we have the same type of thing there. These load-distributing blocks that rest on the I-beams — and again, the I-beams could be dispensed with, the room could be set directly on a level concrete slab on these distributing blocks.

I might mention that we have seen no sensitivity of the residual field to people walking inside the room and so forth. We have had people move around and move the experiments around while we are monitoring the field in the center of the room, and we have seen no sensitivity that we could detect to changing the load or redistributing it on the floor.

Three openings are provided through the shield. Two 6 in. openings are provided for ventilation and cable access. These openings are covered on both stages by open-ended boxes of moly-permalloy to reduce the "bump" in the field caused by leakage at the openings. The door opening is covered during operating by an outer hinged door and an inner sliding door, each covered by an overlapping layer of the shielding material. A detailed sketch is shown in Fig. 4. The doors are made of 1-in. thick honeycomb aluminum. These are clad with a thin aluminum sheet, and the moly-permalloy is applied on each door to the surface that will be overlapping the moly-permalloy that is on the walls. You can see here that the battens are cut short to allow the moly-permalloy along the edges in this overlap region to come in direct contact with the moly-permalloy on the walls of the room. This moly-permalloy is fastened to the doors by means of these batten strips, and also we tried bonding the moly-permalloy around the edges using epoxy. It popped loose in some places, because the material is considerably warped when it comes from the annealing ovens.

We have drilled a few holes around the edge, and put in countersunk brass screws, and the field measurements seemed to indicate that this did not degrade the shield appreciably. If this is done carefully, there is no reason to believe that the material is damaged any more than, say, 1/16 or 1/18 in. away from the edges of

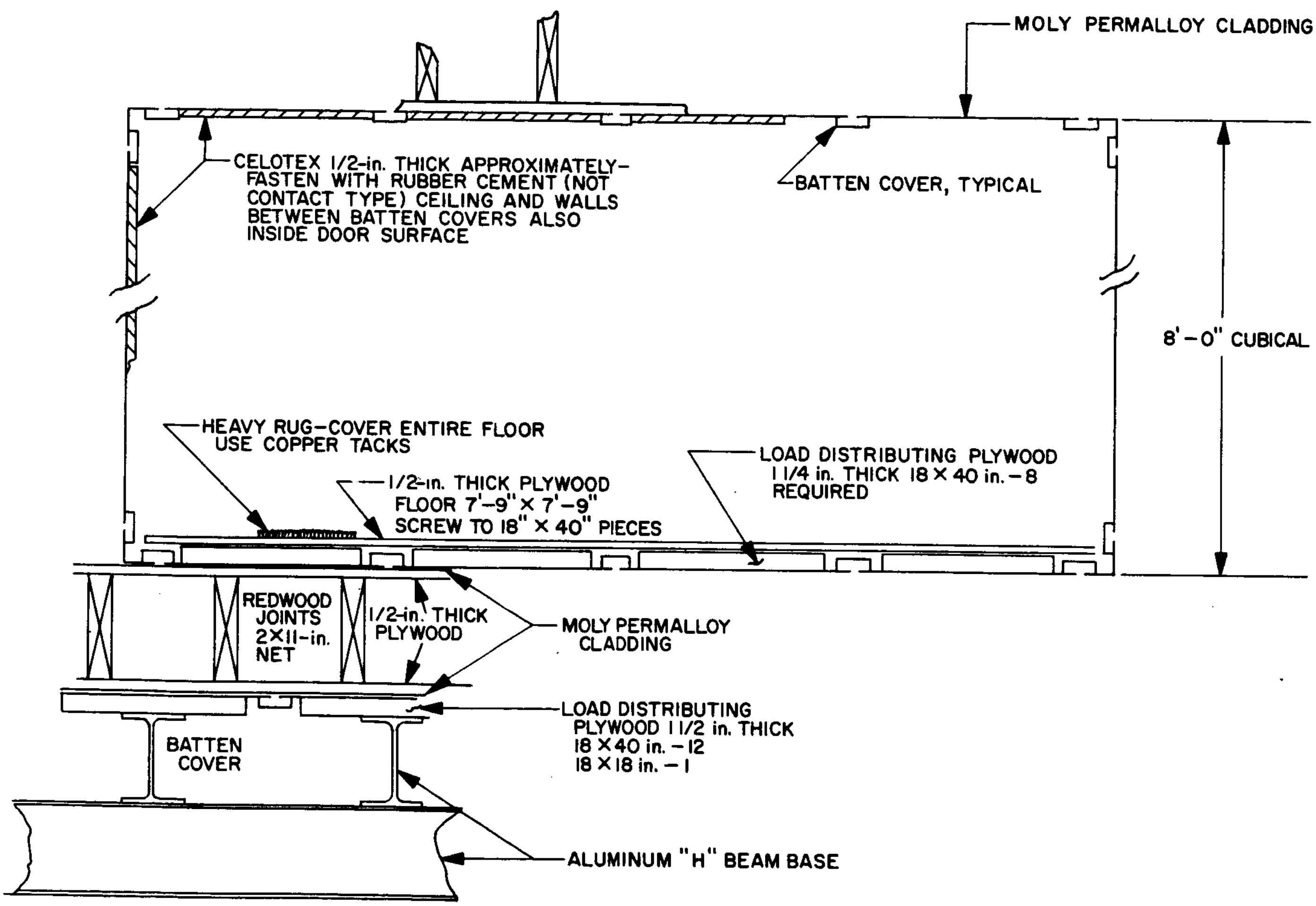


Fig. 3. Section view through magnetic shield room showing inside walls and floor

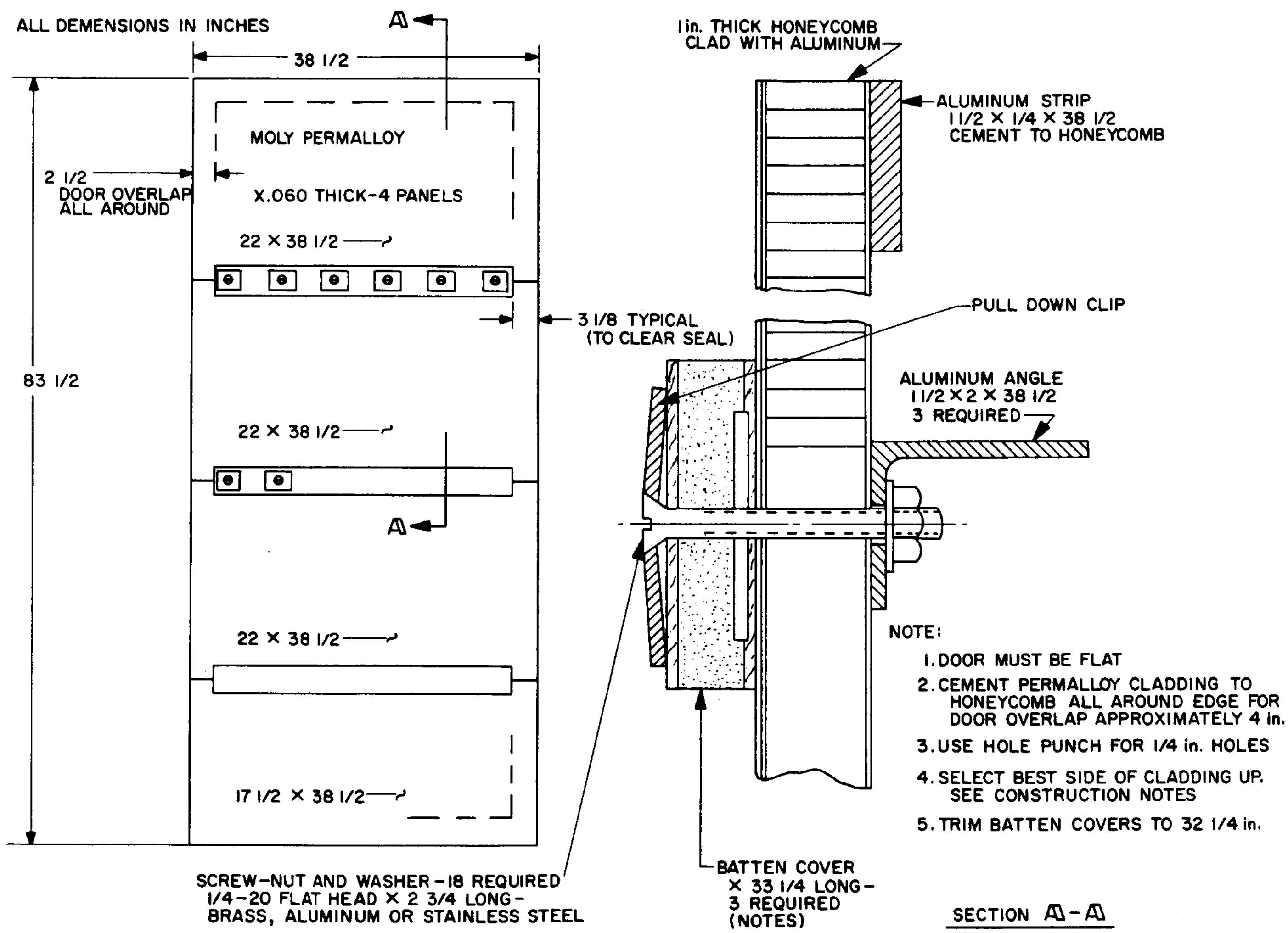


Fig. 4. Inside door of magnetic shield room

the holes, which effectively makes it look like a little larger hole than you drilled, magnetically.

Figure 5 shows photographs of the magnetic shield room during construction.

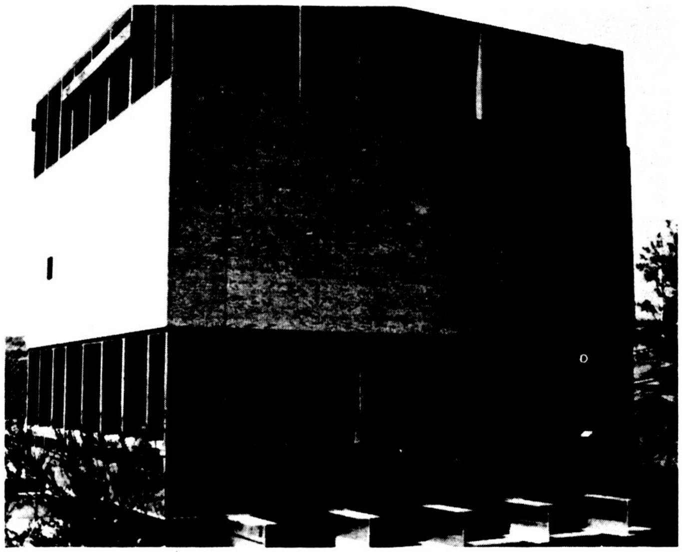
It goes without saying that the overlap area between the door and the walls must be under a certain amount of load to ensure good low-air gap around the edges; that is, at the joint, because the door can't be perfectly flat and neither can the door opening. So we needed a considerable amount of clamping force to force the overlapping regions together.

As this turned out, Patton and Fitch did a real good job here again, and they came up with an ingenious solution: rather than using a lot of clamps, there is a refrigerator seal around the opening, and there is another on the internal edge of the opening. When these doors are closed, the space between the doors is essentially airtight.

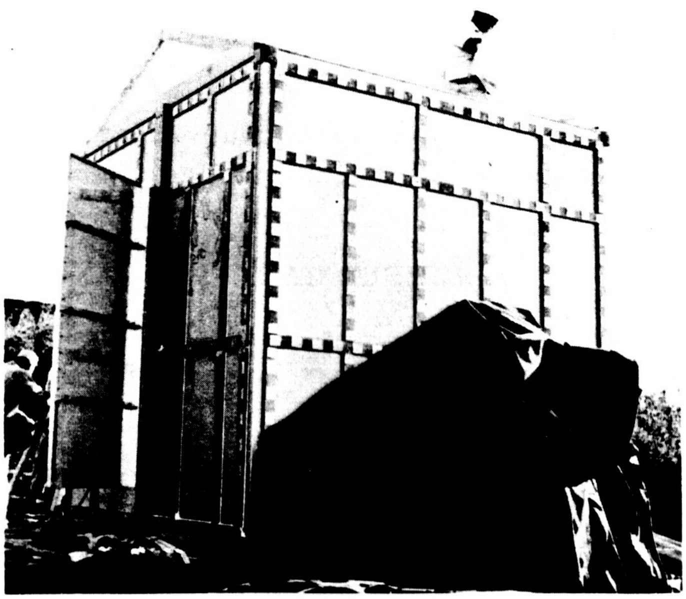
At the top there is an outlet that goes to a vacuum cleaner motor, and with this vacuum cleaner motor we pull approximately 1 lb/in.^{2} vacuum between the doors, which (considering the area of the doors) gives us something on the order of 4000 lb of clamping force on each door. This is nonmagnetic because the vacuum is nonmagnetic. It is sort of difficult to make clamping systems that are not magnetic, but the vacuum cleaner motor was selected because of its ability to pump a fairly modest vacuum with a fairly large leakage rate, and we didn't want to get into the problem of trying to make these seals airtight. So, the vacuum cleaner motor allows a very crude solution here.

PERFORMANCE

Although evaluation of the shield's performance is far from complete, enough results have been obtained to show that the facility meets the objectives mentioned. Accurate quantitative results cannot be obtained until temperature stabilization is achieved, because the character of the residual field is indirectly dependent on temperature. At a temperature of 70°F the field intensity at the center of the room is 18 γ after degaussing the inner shield (S = 2800). Of course, the residual can be changed at will by intentionally "perming" the inner shield. The residual can also be affected by equipment containing magnetic material close to the shield walls, as required by the Mariner Magnetometer calibrations. Because the internal field intensity as well as the gradients depend on the history of the shield, any accurate



a. Partial outer wall complete



b. With moly-permalloy on outer wall

Fig. 5. Magnetic shield room during construction

measurements of performance must be made under carefully controlled conditions and these conditions defined and established in proper sequence. Ideally, the area near the shield is "cleaned-up" and the shield thoroughly degaussed before any evaluation and/or "perming" experiment is attempted. So far, the facility has been under relatively heavy use as an operating low-field facility and the time for extensive experimentation has not been available.

Under practical conditions, the following can be said without qualification:

1. Magnetic noise is less than 0.1γ at all times.
2. After degaussing, gradients are less than 2γ/ft over 80% of the volume of the shield (and inside a 2 ft sphere in the 50 in. coil system while bucking a residual field of 35γ).
3. The residual field is stable to within a fraction of a gamma during stable temperature periods and is reproducible to within 1γ after diurnal temperature cycling and after access-door operations.
4. The inner shield can be "permed" (using a coil) to alter the field intensity inside (e.g. to obtain a passive zero field) without significant degradation of the field uniformity.

Perhaps we should remark on "perming" (inducing a remanence) at this point. Because the inner shield is operating at a point of low induction, the field intensity required to induce a "perm" component sufficient to buck-out the intrinsic residual field is in the range of domain reversibility on the B-H curve. The resulting "permed" condition could not be expected to be very stable. It is, therefore, reasonable (as we have done) to "perm" the shield as far beyond the desired operating point as practicable, then degauss just sufficiently to establish the desired operating point. Because most users of the shield have not required a large volume of controlled field, and because the "perming" operation is a laborious one, the coil system is normally used as "fine" control of the magnetic environment.

When speaking of noise, it is convenient to think in terms of an incremental shielding factor. In general, this parameter is a function of frequency because the incremental permeability and the conductivity of the material vary with frequency. No measurements of AC shielding factor have been undertaken, as such, because of the difficulty of generating a uniform, known fluctuating field of varying frequency and sufficient strength for accurate measurement. Measurements have been made of the ambient internal noise level in connection with testing of sensitive search-coil magnetometers. These instruments have a frequency range from 1 cps to 1 kc. The

noise in the shield is low enough in this range to make accurate determination difficult; however, a 60 cycle component of less than 0.1γ has been detected at a time when the external ambient noise at this frequency was measured at 20γ. This indicates a shielding factor in excess of 200 at 60 cps. Of course, one would expect the shielding to be less for incremental field intensities than for large steady fields because the domain mechanism involved in small changes about any operating point below saturation is primarily of the reversible type. For this reason, one might expect the initial permeability to be an indicator of incremental shielding efficiency much as the slope of the normal B, H curve at the operating point indicates efficiency regarding the constant or steady component of the field environment.

The stability of the residual field in the shield is very good except for temperature dependence. During periods of constant ambient temperature, the internal field intensity does not vary more than a fraction of a γ. During these periods, the field can be zeroed and expected to remain so, as long as the temperature of the shield remains constant. Temperature dependence of the shielding factor has been established by comparison of long-term internal field recordings with simultaneous recordings of shielding material temperature. The diurnal variations of the internal field follow, closely, the material temperature variations. A coefficient of approximately 0.75γ/°F has been established from this data. The reason for this temperature dependence is somewhat uncertain; however, one can expect the shielding material to be stressed because of unequal coefficients of expansion in the moly-permalloy and its wooden substrate. Curves of stress vs. permeability published by Bozorth* show that the stress sensitivity of 4-79 moly-permalloy is very high at relatively low stress levels. An analysis to determine the temperature condition required for tension and compression of the shield shows agreement between the stress hypothesis and the actual measured conditions; that is, increasing temperature causes material compression and increasing internal field. It is also significant that nickel-iron alloys of less than 82% nickel have a positive coefficient of magnetostriction.

CONCLUSION

The room-size shield satisfies all the objectives except for long-term stability. Experiments show that this can be effectively remedied by control of the

*R. M. Bozorth, Ferromagnetism, D. Van Norstrand Co., Princeton, N.J.

temperature environment (experience with the Socony Mobil shield, which is similar and is in a controlled temperature environment, shows stability of the residual to within 1γ over periods exceeding 24 hr).

The total development and construction cost of the shield is not more than $40,000. Because a coil system of equivalent field-free volume with its associated field-control equipment would cost more than $100,000, the shield is an economic approach aside from its superior noise suppression advantages.

The success of a shield assembled from many parts depends heavily on attention to detail. Care in minimizing air gaps along the joints and in handling the material to minimize stress and shock are of great importance during construction. We believe that these two considerations determine the extent to which one can approach theoretical shielding factors, and that they may establish a practical limit for shielding in any application of this size. In addition, we feel that movement of a large shield of this type is very risky and much to be avoided.

The shield has been very successful in the following uses at JPL to date:

1. Precise measurement of output noise, zero-offset, and output calibration of the Mariner Mars sensitive DC magnetometer.
2. Calibration of the OGO search coil magnetometers requiring ambient noise less than 0.1γ from 1 cps to 1 kc.
3. To provide a zero-field of great uniformity and low noise for delicate superconduction experiments.
4. To provide a passive (unattended) low-field environment for long-term stability tests regarding magnetic properties of certain spacecraft components (e.g. Mariner solar panels).
5. AC and DC magnetic mapping of spacecraft components.

Experience with this shield has led to the selection of a shield for the new JPL low-field facility that will be built in the near future.

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OPEN DISCUSSION

MR. GRUMETT: Grumett, of Republic Aviation.

I found the talk extremely interesting, particularly so because we ran into the same sort of problems on a multilayer shield that we were building. We licked some of the problems that you mentioned about not being able to go too low because of residual fields in your shield.

The way that we got around this problem was we overshielded. We put five layers of mumetal shielding, and we permanently wound our deperming coils on the inner three of the five levels. And by going back and forth through deperming, we finally got the innermost shield to find itself in a low enough level. So that by deperming you ended up in this low ambient. We were able to get down to 0.1 gamma for 1/2 day.

MR. CONNOR: Now, this was by deperming the shield that you got down to 0.1 gamma?

MR. GRUMETT: Yes, first we assembled all five levels with no effort to deperm, and in the assembly we had very, very closely wound deperming coils permanently wound. We worked with spherical shapes, and we were only working with a 6-in. diameter, which is drastically smaller than the room you were working with. However, I don't see any reason why you couldn't extend the technique; it would represent a little more investment, but if you had any real need to go lower, it could be done. We wound the coils about 1/8 in. apart on the 6-in. diameter spherical shields. The first three layers were wound this way with the leads brought out.

Then, we very slowly depermed each one sequentially, going back and forth. And we ran into some problems that you wouldn't expect. For example, when you start to deperm down to very, very low levels, the windings in an ordinary pot are too coarse. If you slowly stop your deperming cycle, if you hesitated with your hand on the pot, you'd have to go through the cycle again.

Eventually, we had to use saw-tooth sweeps that we took off a Tektronix scope to bring us down low enough, and quickly cut it off when it got down to zero.

When you get on these real low levels you have to develop new techniques, but you could do it. We got rid of the stressing problem by putting many O-rings around the sphere so the weight of one sphere would not throw all the weight at one point on the sphere. Eventually we will spray latex rubber and have each sphere sort of distribute the loading on the inner spheres. It can be done, and this technique is successful.

MR. CONNOR: The reason we didn't choose to put more than two layers of material — you know, more than two stages on this particular shield — was because, in the first place, we were only aiming for something on the order of 25 to 50 gamma residual in the depermed state. Also, because when you go to more than two shields, then these inner shields are difficult to reach for deperming without special coils built in the device.

With the two stages, one can deperm the outside and the inside, they are accessible. If you have a shield in between with no provision for magnetizing that shield except from the inside or the outside, then (for some reason) if we get a large perm in this then we are in trouble.

Also, I neglected to say that before deperming — the room, that is, we measured the residual immediately following construction without any deperming. We just closed the door and measured the field in the center of the room, and measured on the order of 35 gamma. After deperming the inner shield, we were only down to half that. This seemed to indicate that if care is taken in building the shield it will wind up in a relatively depermed condition. I am speaking now of a multipart shield, and yours was probably a fabricated in one.

MR. GRUMETT: Two halves.

MR. CONNOR: When these parts come from the annealing ovens, they are laid out on platforms and picked up and put on the room, and yet we wound up with

only a field of but twice what the depermed condition was. We feel this is encouraging in working on large shields. It is not easy to deperm a large shield. The dipole moment required for the deperming cycles gets very large.

MR. PARSONS: What is the normal temperature variation that you have to contend with in any day?

MR. CONNOR: Well, it varies. This time of the year I would say it varies somewhere around 20 or 25°F.

MR. PARSONS: And this is 0.75 gamma/°F?

MR. CONNOR: Right. So we are seeing field fluctuations on the order of 15, possibly 20 gamma on some days, diurnal change. These variations, they follow the diurnal temperature shape fairly closely with some lag. During the early morning hours, say, after midnight, the temperature is so constant and the field is also extremely constant. It will run along for hours within less than 1 gamma; but when the sun comes up in the morning, we get a large transient temperature, and our field will follow this change.

Also, I might mention that because we are not conditioning the air and because the ventilation is not very proper, we also have a temperature gradient in the room at this time that we plan to eliminate. And I think it goes without saying that if temperature causes a field change, then temperature gradient can cause a gradient change.

We haven't made accurate enough mappings to prove this, but I think it's a good assumption.

MR. DEBS: Mr. Debs, NASA, Ames.

What is the change in the internal field when you opened and closed the doors? You obviously stressed the panels considerably. And the other thing, is your plywood sealed? What happens to humidity changes?

MR. CONNOR: You mean — say, between the doors being opened and the doors being closed?

MR. DEBS: You set up the deperming and set up the condition, and then you opened the doors and closed them again and thereby stress it, what happens to the residual field?

And the other thing, is the plywood sealed against the humidity?

MR. CONNOR: We usually see the maximum on something in the order of 1 gamma. Say if we are looking at the field and we kill the vacuum and open the doors and swing them out and then close them again, I think that we have seen as high as 2 gamma.

MR. FRANDSEN: There is a long time constant. It seems that if we opened and closed the door pretty rapidly, you aren't going to see a change. If you leave the door open for a while with the edges of the moly-permalloy just exposed to the ambient field, it seemed to pick up a little soft remanence, it seemed to perm up a little bit. When you closed the door again, over a long period of time, maybe 20 minutes, it's back to close to what it was. If you take a small car up close to the room, you might see as high as 5 or 10 gamma. This takes about twenty minutes to decoy away.

MR. IUFER: Iufer, of Ames.

I understood you to say that you believe the scaling factor essentially was independent of room size from small cans on up. Do you care to discuss the basis for this opinion?

MR. CONNOR: Well, actually, I just don't see any reason why the design equation would look any different. What I was trying to get at is, if you have a different power on l or t, the shielding factor is essentially — assuming high permeability, let's put it this way: the induction in the material is a function mainly of the ambient field and the geometry of the room and thickness of the shield, if you have large mu.

I think this is shown if you work out the spherical equations to calculate B_2, for instance. You get a large number of terms. If you find that mu is large, most of these fall out and sort of simplify it down to a function of geometry, and these equations are not worked out with any particular size in mind. They are based on theoretical considerations. Dave Norris here has used these equations, for instance, for a small, rectangular flux tank that he has made for use in his lab. I don't know whether he's planning to say anything on that during this session or not. I think he is.

Bob Patton has also made small shields on the order of 1 or 2 ft, and used the same techniques. So, I don't see any reason why the equations should be different on the scale factor. However, as I said, where you run into problems is the handling of the air gaps and, naturally, as you get larger shields this problem becomes more difficult to handle; to hold the average air gap across a large wall to, say, 0.001 in.

or less, and keep from damaging the material or degrading the material in handling. Bob Patton, I think, did run some calculations on permissible air gaps, and I believe that he found that the average air gap over any one dimension—well, say, across a complete wall of a room—the total air gap should be on the order of 0.001 in. or less, to keep from coming out with 80% of the shielding factor that you are aiming for.

So, in other words, a 0.001 average air gap could cause a detectible change in S, a significant change.

MR. IUFER: Thank you. I was really hoping to find some more empirical evidence. I am associated with the design manufacture of shield cans; there are several sizes involved, and we found the larger the can, generally, the poorer the quality the shielding factor. We found that you could not scale up. The larger you made it you should put in a cushion in the design to cover the degraded performance. Because we made only a few of each kind, we could not use statistics to try to start isolating variables in their design. Our determination is not based on how well it compensated the Earth's field, but it was essentially based on the ability to attenuate very low frequency fields, like 1 cps.

The difficulty in using Earth's field compensation is that internal perms can either add or subtract to the leakage field and give you very significant errors. I guess from your statement, your shielding factor at 60 cycles is something like 200?

MR. CONNOR: I'd say greater than 200. This is a lower limit.

MR. IUFER: And our experience has been that you have been many db higher in filtering at 60 cycles than at 1 cycle. And I might ask: have you thought of rotating your room on casters to find out what its filter attenuation might be for very low frequency?

MR. CONNOR: We don't dare. As I say, because we haven't established temperature stability in our environment, we do have a temperature coefficient. We really haven't made any very accurate measurements of the diurnal variation because of the field, the low frequency field variations. However, as I said, we based our confidence in this respect on Bob Patton's data that has been taken in a temperature-controlled environment. He's made records, say, of 24 hr or longer, where the magnetometer output never got out from between the two lines that represent a

1 gamma change. So, we don't see any reason why ours should be any worse; in fact, I think it should be a little better.

What we did over Bob Patton's design was to increase the space factor. This $(\ell_1/\ell_2)^3$ term. Our case is a little smaller, and we also used moly-permalloy, and in his case he used Mumetal. There is not much difference; the Mumetal is practically as good as permalloy from a permeability standpoint, but it does have a slightly higher initial and maximum permeability.

MR. IUFER: I have one other fairly simple question. You mentioned that you could use this facility for monitoring stray fields produced by spacecraft subassembly. Do you have any number for how large these fields might be before the walls of your magnetic shield room would start following these fields in introducing error?

MR. CONNOR: It is a little difficult to answer what the dipole moment would have to be. Analytically, this would be a very difficult problem. I think, to take a given dipole in the center of the room and say what B would be in the walls. We have a 55-in. Helmholtz coil system that we use in there frequently. We have established fairly large fields in this coil system, and then turned it off to see if we had induced a wall perm with these levels. I think we found that it took something on the order of 10, 000 gamma to get a noticeable change. That's 10, 000 gamma in the 55-in. coil system to get a noticeable change in the walls.

One would have to do some mathematics or geometry, here, to predict what this equivalent dipole moment would be. And I haven't done that.

MR. FRANDSEN: I wanted to make a few comments on Mr. Iufer's question. One, when you scale up the shielding factor, you see that it involves the thickness of the material over the size of one dimension, so I wonder if you were making it thicker as you scaled it up in your experiments.

The other thing was we found when using a search coil inside the room, one has to be careful to measure only magnetic fluctuations, and not the capacitive coupling because of instrumentation gigs. All our search coils are covered with a thick conducting shield.

MR. CONNOR: In other words, cables coming into the room can be antennas radiating electromagnetic noise. So care has to be taken here.

MR. PARSONS: How do you power the coil system, and what is the stability of it?

MR. CONNOR: We have been using a dry cell battery. You see, we are only bucking out in the neighborhood of 10 to 20 gamma, and the sensitivity of that coil is 2.5 μa/gamma. We are using the dry cell with a ten turn potentiometer. The only requirement is that it have stability of 1 part in 100.

For magnetometer calibration, normally, we use a Princeton power supply, and we put very accurately known currents through the coils, and these coils are very carefully calibrated both in the room and out of the room. We usually do it in both places just to see what the wall effects might be, what percentage the presence of the wall surrounding the coil may change the calibration.

MR. PARSONS: When you get in the new facility, what would be the dimensions of this new shield room?

MR. FRANDSEN: The ground is broken for the building. The high bay area is a 23 ft cube. We haven't really decided on the size of the room, but I think the largest it will be is 15 ft on the outside and 12 ft on the inside cube.

CHAIRMAN GAUGLER: Keith Lamson of Allegheny Ludlum was telling me that he was somewhat concerned that they make this material but they don't make rooms — and so when you go out and buy a room someplace you've got to be sure you get the right kind of specifications, because annealing big sheets of this type is not too easy. And I suppose that the other mechanical problems associated with handling it are not too easy, either.

MR. CONNOR: I would say that if you took the same batch of material and just had two different crews working on handling and constructing it, you could come up with pretty divergent results. On this room, the only people handling the materials, putting it on the rooms, were the magnetics people, magnetics technicians. These were being supervised and also helped by the engineers that work with magnetics. We didn't let the sheet metal people or anybody come in and do this work. We did it ourselves. We told the carpenters and cabinetmakers that they could build the box, but we told the sheet metal people that they couldn't touch our metal, unless they wanted to take the responsibility for sending it back and having it annealed if it was damaged. They decided that they'd better let us handle it.

MR. DROLL: Droll, from Ames.

Do you still, in this new room, propose to shoot for this 30-gamma ambient?

MR. CONNOR: Yes. It is a little difficult to answer that. It depends on what's available, you know, from stock and the materials selected. If the t, thickness factor, for example, happens to be a little different from what we want, it might go a little over 30 gamma; but we do hope by possibly increasing the spacing factor to offset things of this sort, to still maintain it below 30 gamma with a two-stage shield.

MR. DROLL: But, at any rate, you don't intend to shoot for a lower field than what you've already achieved in the smaller room?

MR. CONNOR: No, I don't believe so. It seems too easy, by inducing the perm for instance, to establish a fairly stable zero field. If we want a zero field, and if we do have temperature stabilization, then we can get very stable fields at almost any value we want, including zero, by perming. So, for this reason, I don't really believe it's necessary to go to the expense and extra design effort to try to improve on these figures. It is just my opinion.

MR. BROOK: Brook, Republic Aviation.

How do you propose to stabilize the temperature in the room? You mentioned before air gaps of 0.001 or 0.0001 in. would be the maximum. Are you going to have air conditioning ducts of some sort, or some sort of a new perforation technique?

MR. CONNOR: What I am speaking of is having a constant temperature both on the outside and inside of the room, so that the structure and the shield are not undergoing temperature changes. The easy way to do this is just to place the whole facility or shield in an area that is air conditioned to a fairly good degree.

Then, the ventilation air — we do blow ventilation air into the inside of the room with a small blower. We would take the air that we blow into the room from the surrounding, that is, the air conditioned structure that the room is in.

MR. BROOK: How do you blow the air into the room? I mean, right through a wall, or through a duct or vent?

MR. CONNOR: We have a 6-in. opening in the roof, that is, the top of the shield. These openings cause very small bumps in the field. If you run a magnetometer along, you have to get within 1 ft of the wall to even detect a bump on these 6-in. openings. But, we have gone ahead and put shallow open-ended boxes over these openings, just to cut down the flux leakage here. We also have another 6-in. opening similar to that on the side of the shield for cable access, so we can run cables, tubing for cryogenic liquids and this sort of thing.

MR. PARSONS: Do you find that you have any biophysical or morale problems with the man that's inside this room? How long does it take to get him out?

MR. CONNOR: The only time it bothers is when the candles go out. Since then, we have installed electric lights. We use these in a manner that if we don't see any problem — that is, the experimenter doesn't see any 60-cycle field is bothering us — then we leave the light on. If the experiment is critical, if this comes into play, we can turn that light off and let the fellow light some candles and grope around in there.

We hope to do a more professional job on this lighting in the new and more permanent facility.

VOICE: Do you use incandescent or fluorescent lighting?

MR. CONNOR: We used an incandescent bulb in a ceramic socket. This is placed over towards one of the corners to keep it far away from the center. The wires are twisted to keep down the current-loop fields. It is hard to detect the effect of the lights out at the center of the room.

Of course, if someone were working on something and he had to make measurements over closer to the lights, then he could get into trouble.

VOICE: Have you considered optical fibers for getting a light arrangement?

MR. CONNOR: Well, yes, we have. We looked into this. We also looked into this sort of thing for remote readout of instruments. You know, you certainly can't put a dial indicating instrument — I mean a voltmeter or something — in there, because they have large magnets in it, and many times the fellow working inside would like to see what the magnetometers say, and the magnetometer is out in the control area. He, of course, has communication and he has to ask what it is doing, and the other fellow calls out numbers to him; so we would like to get a remote reading system that is nonmagnetic.

The most promising thing, so far, for consideration of expense and so forth is to sort of clean up a digital readout module, the kind that comes in the digital voltmeter. Some of these are surprisingly nonmagnetic. To start with, they just need a change of a few parts here and there to make them nonmagnetic. So, at least you would have a voltmeter readout in the shielded room that would be nonmagnetic.

The fiber optics, we didn't look into them thoroughly enough to make a definite conclusion. We decided against it, but whether this was based on enough

information to discourage anyone else or not I don't know. I wouldn't want to discourage them.

MR. IUFER: If I could make just a design suggestion, we did some work with open-end cylinder shields, and for the order of correction you would like, you might consider using a two-layer cylindrical shielding on your access hole that has a length to diameter ratio of four or better. Then you can use a spotlight, shine it down the pipe and onto a conical mirror that would distribute the light in the room or on a screen. You can get light in without getting flux with it.

MR. CONNOR: I think that is a good suggestion. I don't know what trouble you might get into with wave guide effects or high frequencies being piped in on this pipe thing — I really don't know if it would be a problem or not.

MR. GORDON: What's the procedure in perming your walls to achieve a zero field?

MR. CONNOR: What we do is measure the residual field. Let's say if you are interested in the north-south component field residual. We orient the coil so that its dipole moment lies north-south, and we feed in a DC current. After this current is turned off, we measure a field much larger, but in the opposite direction to the residual you're looking at. If we want to come back to zero or some other field value intermediate to that, then we apply an AC deperming field and we ring this down, decrease it very slowly, and I think it was mentioned before that you get into trouble on the quantitization of say, a Variac or something — sometimes you have to do this in two or three stages to come down with a fine ring down. By trial and error just keep applying a little larger maximum on this deperming field until, when we get down to zero, the field is what we want.

It is, admittedly, a trial and error thing, but we have found that it is fairly repetitive. I think that after a person has done this a few times he could hit it in two or three tries, a person could predict fairly accurately what he needs. So, this is what we have been using. There is a slight amount of cross-talk. In other words, changing the field north-south, it may change east-south by some percentage, say 10%.

N66-11286

PROLONGED EXPOSURE OF HUMAN SUBJECTS TO MAGNETIC FIELDS OF LESS THAN 100 GAMMA

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I would like to make these remarks in the form of discussion remarks and not in a formal presentation. I have a few slides along with me and I would like to use them as I'm going along to explain our results.

We had the cooperation of the Naval Ordnance Laboratory, and it was in the first experiment with Mr. Parsons who is in the audience. In the second experiment, it was Mr. Ford who helped us considerably in our exposures of man.

We exposed in two experiments – in the first, two men; in the second, four men – for a period of 10 days continuously to a field between, I would say in the center of the coil system we had nearly zero gamma, and at the edges of an 8-ft central area we had about 100 gamma. So, we had a field with a gradient of about 8 gamma/ft. I would like to stress that our people were continuously in this field. That means that they were continuously in the room for 1 mo.

We had a shakedown period and control period of a little bit more than 1 wk before we started the experiments, and nearly 2 wk continuous exposure to a low field, and then a 1 wk control period. So, all in all, we had, in the first experiment, two boys, and in the second experiment, four boys, in an area 8 by 8 ft. They slept there and ate there, and so on, and never left this room. We used two brothers for medical reasons.

The first slide shows the room. Some of you will notice the coil system, which is fairly old and was not (of course) constructed for this purpose. It was 30 by 30 ft in area. Mr. Ford changed it so that it would automatically maintain a nearly zero value at the center. It did not change by more than 1 gamma. The boys stayed in the 8 by 8 area in the middle. Here are the sanitary facilities, Navy style.

All the equipment, of course, was made of nonferromagnetic material. We found that the floor had too high values and we had to tear out the floor before the experiments. Otherwise, we kept every other little piece of ferromagnetic materials out, the buckles of the people were checked, even the hinges on the glasses were checked for ferromagnetic material. We were very careful to keep the values down and noticed if any material was introduced into the room with the magnetometer.

The next slide is one of our experimental arrangements for measuring flicker fusion. We presented a number of tests to the people that I will mention later. Perhaps, with the slide here, I will explain this one test.

On a 3 by 4 ft area we were able to expose a flickering area with a certain frequency. The frequency could be changed from 5 to about 30 or 40 cps, and the man was sitting here in front with a bit in his mouth so he couldn't move his head. The whole thing was covered with a black cloth that has been removed in this picture to allow an inside view.

We also had the possibility to project a flickering light spot to any part of the background of the eye. We had two more flicker possibilities, flicker fusion tests, built in. We found certain changes in the threshhold of flicker fusion in our first experiment, so in our second experiment we stressed flicker fusion very highly and had independent measurements of flicker fusion installed.

I would now like to mention the kind of tests we made. We made, of course, a number of physiological tests; we measured the blood pressure, we measured the body temperature daily — morning and evening. All our equipment was changed so it had no ferromagnetic components. For instance, the normal arrangement for measuring blood pressure had to be considerably changed to fit our purpose.

I just want to mention, in connection with the last talk, that we signaled our results out by light signals. For instance, in the psychological experiments, if a man saw or didn't see a light, he would signal out, interrupting a light beam, and in this way we had all our recording equipment outside far enough not to interfere with the field inside the coil system.

We also had other signaling devices; for instance, pulling a string that gave us a signal to the outside. We will probably proceed a little bit different in the future, but there are many possibilities to signal to the outside the results received inside the coil system.

Besides the usual physiological experiments, we had a number of optical tests; for instance, aligning of lights along a certain stretch. We had, for instance, dark adaptation that was measured during the experiments. We had EEGs, which are electroencephalograms. Not only a normal EEG was done, but stimulated EEGs were given. That means we stimulated the men by a flickering light and measured the changes in the first 1/4 sec period after the light flash.

Then we measured the EKG, electrocardiogram, not only under normal conditions, but also under working conditions. We had these people work by using some kind of step ladder and also by lifting some weights.

Working with us we had Dr. Ralph Rytan, from the Neuropsychological Clinic in Minneapolis. Dr Rytan installed about 25 different tests in the form of a modified Hallstead battery. He uses the tests, normally, to find brain damage in shock wounds and under other conditions; for instance, cerebral cancer, under conditions where the brain is damaged. He applied this method of testing before we started the tests, during the tests, and also after the tests.

The boys went through a careful evaluation in this neuropsychological-physiological testing. Most of them were physiological tests, but some of them had a certain psychological application. We subjected these boys to a continuous testing period of at least 6 to 8 hr/day. Of course, we did not overtest them. This mistake is often made. We gave them in-between recreational periods, but we tested them as much as possible.

The results can be summarized very briefly. They were negative for nearly all of our tests. That means there was a slight change in weight of the boys. They nearly all gained weight about 5 lb, but this was just keeping active boys on an 8 by 8 ft area; and we gave them all the food they wanted. As a result, they increased their weight. However, this is minor. We only noticed in five of the exposed people and probably also in the sixth man there was a change in the flicker fusion threshhold.

I have a diagram of all of the days. There were about 45 days. We had a control period, an exposure period, and a control period again.

You can observe a catoptric flicker; that means flicker at the rim of the retina. We measured certain values before and afterwards; during exposure the values went gradually down. In the most expressed case, the values dropped down to 20 or 25% of the original value; and in all other cases, with one exception, they came at least down 20% from the original value. They came down gradually. There were no rapid values coming down here, and there were no rapidly increasing values.

The people were not aware that the magnetic field was changed, here, from environmental field to the low field, and they were not aware that we changed back at this point. The values came slowly back. It took about 12 hr before the people returned to their base value. One man showed some wavering. These are our main findings to this point. We, of course, tried to locate this effect and we looked up literature on flicker fusion. There are close to 1000 publications out now. We

worked our way through as well as possible, and found that the people working in this field had no idea if the effect is peripheral — that means in the retina of the eye — or if it is central. They are still trying to locate the effect at either place.

We thought, coming fresh into the field, we should just state our results and not get into discrepancies. But we believe that the next checks should consider the effects very close, or the testing procedures very close, to flicker fusion and probe a little bit more in this area because it may be that a very sensitive peripheral flicker fusion may give us a wedge into the physiological effects, not only of this low field but also of the geomagnetic field. We are interested in the effects of the geomagnetic field.

As you know, there are experiments going on at a number of places; mainly at Chicago. Dr. Brown at Northwestern believes for certain that the geomagnetic field has an effect on all living beings. He was doing his experiments mainly with Paramecia and mud snails as they creep out of a hole and follow a certain direction that may be influenced by the lunar time and also the solar time. It is quite complicated to follow his reasoning, but no doubt he has a certain influence of the geomagnetic field established in his experiments.

Other people have followed his lead, and we can say right now that man lives not only in a gravity field but also in a magnetic field with certain directional influence on living matter.

We have also transgressed the normal exposure values for a living matter in magnetic fields. We used the facilities of the Naval Research Laboratory and exposed living beings up to monkey size to very high fields. Our highest field strengths at 160 kilogauss, and we used a modified Bitter magnet. I won't go into that right now because it is actually a conference on low fields, and I will not mix it up with the results of the high fields.

We believe more and more that the magnetic field has certain influences on man and also on all living beings. I believe before we go to the moon and before we expose our astronauts to lunar environment for a long period of time, we should, under all conditions, know what these low fields are doing.

Our only values available right now on the lunar magnetic environment are the values published by the Russians in one of their lunar crash probes. Obviously, their magnetometer was not functioning up to impact. At least in certain heights, a few thousand miles, I believe they measured a field lower than 1/400 of the Earth's field. We have to expect low fields on the moon; it is in the range of about 100

gamma. It may be different close to the surface of the moon, we may have ferro-magnetic materials there; so it may be different in immediate surface contact. We have to expect low fields there.

OPEN DISCUSSION

MR. PARSONS: Since the last time I spoke with you, I have contacted another man, a Dr. Howard Andrews of National Institute of Health, and he tells me of experiments that he was associated with, where the human head was exposed to fairly low fields, but in the frequency of 10 cps. He described a situation where the human brain apparently had a natural frequency in this order in which it would attempt to track this ambient signal. Then, if you varied the 10 cps down towards 9 or 8, the brain would try to follow down until it got to a certain threshhold, and then it would break away.

He indicated if you held the person there at that threshhold level for very long, he would become nauseous, sick, and so on and so forth. I wasn't familiar with this. Perhaps you are? I just thought I'd mention it.

DR. BEISCHER: Yes, I am.

MR. PARSONS: One other thing that I picked up on biomagnetics is a paper given by a man in Paris, I don't recall his name now, concerning the divining rod.

DR. BEISCHER: Yes, I know him also.

MR. PARSONS: This man was claiming that a very small field change was sensed by the muscle system of the man and this is what caused him to dip his rod at the proper moment. They used an artificial coil with a field generator that came on the man's body, and he'd walk by with the current on one time, with the current off one time, and so forth. He, of course, was not told whether the current was off or on. And sure enough, there was demonstrated 100% accuracy in the right range of field changes. I am thinking here of a 150-gamma change — was all it took for him to sense the difference and force his rod to dip.

DR. BEISCHER: Yes, he got in contact with me also. We have to be very careful in this whole field, in connection with quacks. This Frenchman is not one at all. We have to be extremely careful. There was a time that was characterized by Mesmer. He was an Austrian physician who worked mainly in Paris and who had a lot of female customers who believed he could cure all kinds of illness by a magnetic

field; he applied it and obviously with some results. Because of him, now we have to deal with Mesmerism, and this is plain deceiving people.

In our experiments we tried to stay absolutely to proven scientific methods. This is a field of low forces and you may be easily misguided, just as in g-zero experiments, we do similar experiments in g-zero. You may be easily misguided. It is absolutely necessary that institutions and people of scientific schooling get in this field and establish what actually happens here. Of course, the more people that work in this field, the more we would hope to have results available.

I would like to mention here also the observations made in magnetic storms — there are about five publications in the literature in which people found that during magnetic storms, where the Earth's normal field changes in maximum of about 100 gamma — there are physiological changes. It was mainly Baker at a New York State mental hospital who claimed that during this time the check-in rate of patients in his hospitals — he is supervisor of a number of hospitals — is much higher than under normal conditions. Also, other people believe they can coordinate these magnetic storms with higher accident rates, higher rates of suicide, and a number of other things. All this statistical coordination may be valuable in certain cases.

You can go very far in this kind of correlation attempt, and there may be something to it. With the magnetic storms you have also changes of the influx of radiation. We planned for some time to use the system at the Naval Ordnance Laboratory to make artificial magnetic storms and expose our people to these artificial storms. Then you can work in a magnetically-quiet period outside and have only the influences of the magnetic storm or magnetic changes on the man. There are a number of experiments you can do in this field, quite generally.

MR. IUFER: I would like to make a comment. At Ames, Dr. Conley, Mr. Mills, Mr. Droll, and myself are doing research on small mammals in low-field areas, and there seems to be, thus far, statistical correlation. We had results right off the bat that we traced to changes in the oxygen concentrations, temperature, and everything else. After these variables were controlled, we found that in small mammals the immune reaction was depressed when the mammals were in a very low field.

In our case it was 50 gamma or less. We were using mice, standard biological control techniques. The field of biomagnetics is rather extensive. There are, I think, more than 200 or 300 publications on this subject. Most of the work has been done, however, at high field levels rather than low.

There is another experimenter at Ames, his name is Dr. Gualtierotti, and he has demonstrated that in a field of somewhat higher than 500 gauss, the sodium transport mechanism of a frog skin fails. This happens to be a reproducible experiment with quite good control, and it is also cyclic. You can program the field above and below this point, and this biological transfer of sodium through a membrane in the abdominal skin of a frog will keep repeating until the skin dies.

I should also like to point out that these effects seem to be very small, and designers who are trying to get dirty magnetic hardware passed through should not rely on the confusion of the operators of magnetic laboratories.

DR. BEISCHER: I would like to mention, in connection with what you mentioned, that we exposed a bread mold to such low fields below about 50 gamma. We used a variety that grows in spurts it is called the glauc-mutant of neurospora and we found, in this field in comparison with the normal geomagnetic field, definite differences in growth speed. There seems to be something — of course you always have to narrow down — that there may be little environmental differences inside the box. We, of course, had our normal outside controls. We had them in copper boxes and there may be little things that you have not observed. It is quite a trying field.

VOICE: Some workers in biomagnetics have noticed a geographic direction in the movements of migratory birds. Would you care to comment on that? They claim that they move almost exactly along the magnetic line.

DR. BEISCHER: The Navy spent many millions of dollars on these migrating of birds experiments. It was mainly the Naval Research Facilities who, over years starting about 1920 to 1940, were very intrigued by the possibilities that birds use the magnetic field in their migrating paths. Numerous publications are available; I would say there are close to 100 publications in this field. I believe the general consensus right now is that birds use the field only indirectly. That means in conjunction with something else that we do not know. For instance, it is believed that the birds, by moving their wings through a steady magnetic field, generate electricity that they use in gauging the field and also in gauging the direction of the field. In general, all this (I would say) is negative. Right now we cannot say for sure that the birds use, during migration or homing, the magnetic fields of the earth, or only in conjunction with something else that we do not know.

There are experiments where birds and bees and other insects land only on a solid surface in a certain direction. Again, an indication that there is a sensing

organ for the magnetic field. Of course, if the birds would use this kind of sensing, then we would have a very small sensing unit, and actually we would be very much interested to know what it is.

MAGNETIC MAPPING OF MARINER MARS ASSEMBLIES

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INTRODUCTION

The purpose here is to present the methods used in measuring the magnetic fields of Mariner Mars assemblies. For this discussion, the term "assembly" is used in a broad sense to refer to various pieces of the spacecraft both large and small. The prime reason for making these measurements was to predict and control the magnetic field at the location of the flight magnetometer.

The measurements were made at the JPL Mesa inside the 12 ft spherical coil system. The coil system provided a near zero magnetic environment so the effect of induced dipoles would be negligible.

PERM FIELD MAPPING

The "permanent" magnetic field of a typical assembly was determined by performing three separate tests. Each consisted of rotating the assembly while a single axis test probe monitored the radial field in the plane of rotation. An X - Y plotter was used to record the radial field versus angle of rotation. The three test rotations were made about mutually perpendicular axes. These axes form a right hand coordinate system (1, 2, 3) that was assigned to the assembly for the convenience of this mapping. Generally, the coordinate system was assigned in such a way to permit the assembly to be easily rotated about any of the test axes without placing undue stresses on sensitive portions of the assembly and without danger of it tipping over. Plastic spacer blocks were used, where necessary, to attain perpendicularity between the three planes of rotation. Once it was decided how the coordinate system was to be assigned, a sketch was made showing the test coordinates in relation to the physical shape of the assembly.

If the assembly has a single dipole source located at the origin of the test coordinates, then at least two of the X - Y plots will be 1 cycle of a sinusoid.

However, many assemblies have multiple sources, but they are generally not of equal moment. So at distances large compared to the source separations, the predominate source makes the radial field approximately that of a dipole.

There is a special case involving sources having approximately equal moments. When a pair of equal sources are directed antiparallel, the dipole term in the radial field expression vanished and the quadrapole term becomes significant. This occurred in about 2% of the Mariner Mars mappings. For all other assemblies, if they exhibited any appreciable field at all, it was essentially dipole.

Figure 1 represents a typical assembly having dipole moment, M. A standard test 1 was defined as a positive or clockwise rotation about the +1 axis. The starting point for this rotation was with the +3 axis pointing along the axis of the test probe a distance, r, away. At the completion of test 1, the orientation for test 2 was achieved by rotating the +2 axis 90 deg into the +1 axis. A standard test 2 then consisted of a positive rotation about the +2 axis. Similarly, the orientation for a standard test 3 was achieved by rotating the +3 axis 90 deg into the +2 axis. Test 3 then consisted of a positive rotation about the +3 axis.

Figure 2 shows the form we used, simple as it is. What I wanted to show was that the test coordinates are printed on the form. We then make a sketch, in this case a module, showing how it was oriented in relation to the test coordinates. Now, it is sufficient, actually, to have one sketch, but two are a little bit easier to visualize.

Attached to this form, which was distributed to the cognizant engineers and to whomever else required copies, was a copy of the X - Y plots shown in Fig. 3.

If the unit has a net dipole moment, at least two of the plots will be one cycle of a sinusoid. The peak gamma obtained in test 1 was called B_α and is proportional to the projection of the dipole moment M into the 2 – 3 plane $\left(\sqrt{M_2^2 + M_3^2}\right)$. The peak gamma obtained in tests 2 and 3 were called B_β and B_δ respectively. They also are proportional to the projection of the dipole moment into the plane of rotation. The constant of proportionality in all three cases is $1/2\ \pi\ r^3$. Forming the expression

$$\frac{B_\alpha^2 + B_\beta^2 + B_\delta^2}{2}$$

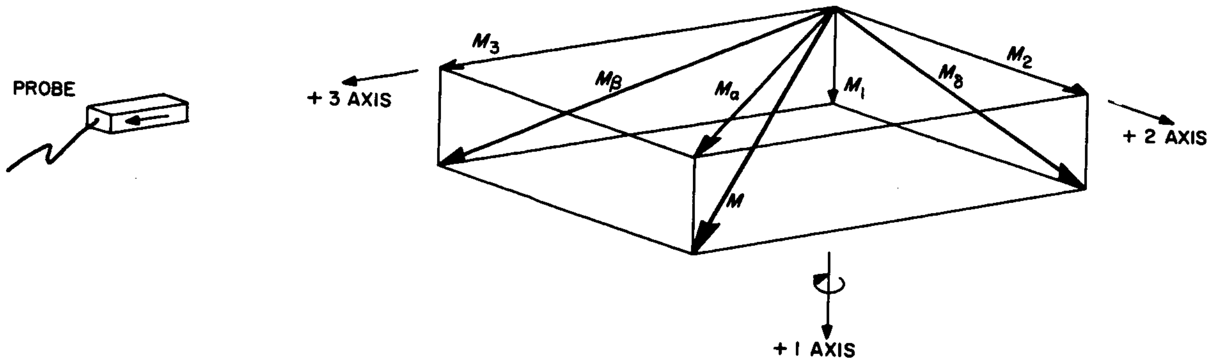
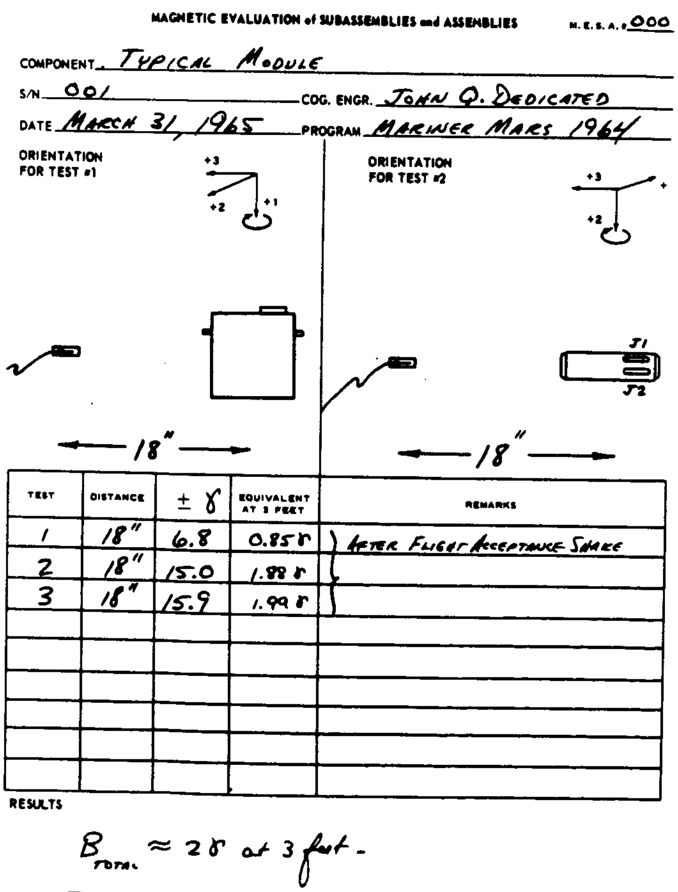


Fig. 1. Representative assembly shown in test 1 orientation



MAGNETIC EVALUATION of SUBASSEMBLIES and ASSEMBLIES M.E.S.A.# 000

COMPONENT Typical Module

S/N 001 COG. ENGR. John Q. Dedicated

DATE March 31, 1965 PROGRAM Mariner Mars 1964

ORIENTATION FOR TEST #1 | ORIENTATION FOR TEST #2

18" | 18"

TEST	DISTANCE	± γ	EQUIVALENT AT 3 FEET	REMARKS
1	18"	6.8	0.85γ	After Flight Acceptance Shake
2	18"	15.0	1.88γ	
3	18"	15.9	1.99γ	

RESULTS

$B_{total} \approx 2\gamma$ at 3 feet.

Fig. 2. Sample form for magnetic evaluation of subassemblies and assemblies

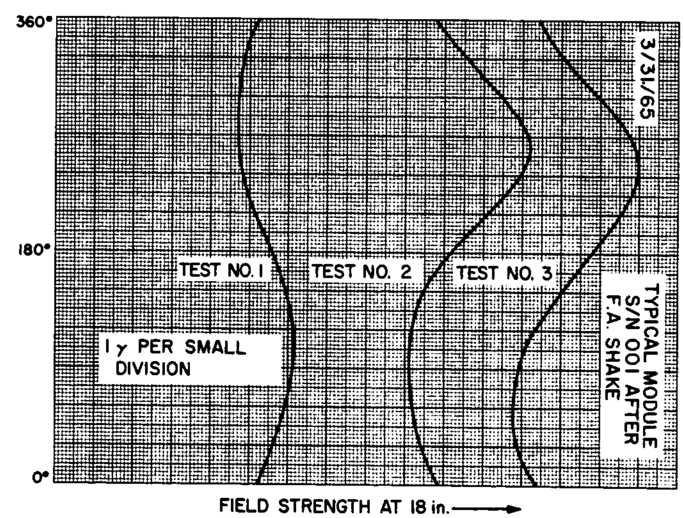


Fig. 3. X-Y plot of magnetic field of typical module

We get:

$$\sqrt{\frac{B_\alpha^2 + B_\beta^2 + B_\delta^2}{2}} = \frac{\left(\frac{\sqrt{M_2^2 + M_3^2}}{2\pi r^3}\right)^2 + \left(\frac{\sqrt{M_1^2 + M_3^2}}{2\pi r^3}\right)^2 + \left(\frac{\sqrt{M_1^2 + M_2^2}}{2\pi r^3}\right)^2}{2}$$

$$= \frac{1}{2\pi r^3} \sqrt{\frac{\left(M_2^2\right) + \left(M_3^2\right) + \left(M_1^2\right) + \left(M_3^2\right) + \left(M_1^2\right) + \left(M_2^2\right)}{2}}$$

$$= \frac{M}{2\pi r^3} = B_{max}$$

so:

$$B_{max} = 0.7 \sqrt{B_\iota^2 + B_\beta^2 + B_\delta^2}$$

The dipole moment M can be computed by the relation $M = (B_{max})(2\pi r^3)(10^{-9})$. Where M is in weber-meters, B_{max} in gamma, and r in meters. Probably the most useful way of describing M is in component form. M_1, M_2, and M_3 are found by solving a set of three equations.

$$M_1 = \sqrt{B_{max}^2 - B_\alpha^2}\left(2\pi r^3 \times 10^{-9}\right)$$

$$M_2 = \sqrt{B_{max}^2 - B_\beta^2}\left(2\pi r^3 \times 10^{-9}\right)$$

$$M_3 = \sqrt{B_{max}^2 - B_\delta^2}\left(2\pi r^3 \times 10^{-9}\right)$$

Because the expressions for the components of M involve radicals, Table 1 is used to determine their sign.

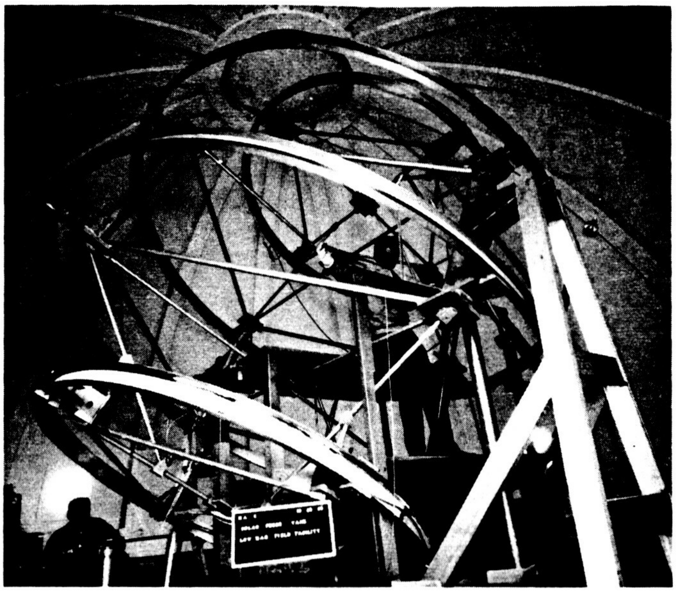
Table 1. Determining radical signs

Quadrant in which positive peak occurred			Sign of Ms		
B_α	B_β	B_δ	M_1	M_2	M_3
I	I	II	−	+	+
II	II	II	−	+	−
IV	IV	IV	+	−	+
III	III	IV	+	−	−
IV	I	I	−	−	+
III	II	I	−	−	−
I	IV	III	+	+	+
II	III	III	+	+	−

There are only eight rows in this table. This is a reflection that the M vector must lie in one of eight quadrants. These components of M can be used to estimate the field contributed to the flight magnetometer by the assembly. It can be argued that it would be easier to make this estimate if the test axes had been chosen parallel to the spacecraft axes. However, early in the program the location of some packages was still subject to change. In addition, the use of spacecraft coordinates complicated many mappings by requiring that the assembly be supported at an odd angle. It was therefore decided to assign a set of test coordinates for the convenience of the mapping.

Figure 4 shows photographs of the coil system. It is a single-axis spherical coil system. It looks like it might be a Braunbek, but it really isn't. For some obscure reason the coils are on the surface of a sphere. This requires us to juggle the turns ratio. The number of turns in the large coils to the number of turns in the small coils should be, ideally, 1.466 to 1 to give us minimum gradients.

This we don't quite have. We have actually juggled the turns — the ampere turns — with a shunting potentiometer across the two big coils to give us the desired ratio. You can't see the turntable here, but on that wooden table in the center — on top of it — there is a Micarta turntable on brass bearings, and that's where the



a. Viewed from northwest

b. Viewed from southwest

Fig. 4. Spherical coil system oriented in direction of Earth's magnetic field

package is placed while a technician supports it there and turns it for the test. There is a little wheel that rides on the edge of the turntable, and there is a shaft coming down here to a potentiometer (10-turn potentiometer on the floor) and that gives us the voltage proportional to the angle of rotation to the table.

We also have a shaft coming off at an angle to a motor that can turn the turntable, but we don't use that because of the delicate nature of the flight hardware. We felt that it was better to have a man up there to turn it by hand.

Figure 4b is practically the same thing but a different view. The single-axis test probe is shown and normally it would be pointing toward the package in the center of the turntable. In the figure, he is just sniffing a package.

Once the maximum field from an assembly is determined, the question of magnetic stability arises. In the Mariner Mars program assemblies were mapped both before and after type approval shake. This was an attempt to gain knowledge of the magnetic stability of a package and was based on the fact that all assemblies underwent the same type of shake. But it turned out that there were gross differences in the steady state field of the various shake tables used. So, on the assembly level, the magnetic stability results were uncertain. However, the question was resolved on the system level by mapping a complete spacecraft before and after shake – also by comparing the prelaunch spacecraft mapping with that obtained during the near Earth spacecraft roll maneuver.

CURRENT LOOP TESTS

Measurements of magnetic fields because of DC currents were performed in the same manner as perm field mappings, but with the required cables supported from above, along the axis of rotation. It was first determined that the OSE cabling connectors were nonmagnetic.

Measurements of magnetic fields because of step functions, impulse functions, or AC currents below 5 cps were repeated three times – one time each in the three mutually perpendicular test orientations. The time varying nature of these signals did not permit the unit to be mapped by rotation. Field changes were read on the front panel meter of the test magnetometer. The meter movement does not respond to AC fields much above 5 cps. The measurement of fields above 5 cps was not

required for the Mariner Mars Program. To minimize the measurement of currents in the supporting cables, these cables were directed parallel to the sensitive axis of the test probe and were kept as far from the test probe as practical.

The results of current loop tests on assemblies were later verified on the system level by monitoring the flight magnetometer as the spacecraft was put through various modes of operation.

GENERAL COMMENTS

The major sources of error in these mappings were: (1) distortions to the predominately dipole field because of multiple sources, (2) not achieving exact perpendicularity between the three axes of rotation, and (3) not rotating about the "magnetic center" of the assembly. Because of these effects, the results were typically in error by ±5%.

The maximum package to probe distance, r, used in mapping Mariner Mars assemblies was 3 ft. This was a limitation imposed by the supporting structure in the coil system. For a few of the tests it would have been desirable to use a larger r, but this was not a serious limitation. If, however, a large enough zero field environment had been available early in the program, there is an additional test that we would like to have performed to simplify the problem of total field prediction. This would be to simulate the spacecraft location of each assembly relative to the flight magnetometer and then read the field with a three axis test sensor. The field would also be read with the package at infinity – the difference in the two readings being the assembly's net perm field contribution. One could then estimate the net contribution from all assemblies by simply summing the effects of each. This is based on the gross assumption that packages are not magnetic enough to cause large induced dipoles in adjacent packages. Assemblies are built with nonmagnetic components whenever it is possible to do so without compromising design reliability. So, while the assumption is not completely valid, it would seem to be an adequate method for the purpose of estimating total field.

OPEN DISCUSSION

MR. IUFER: What reference did you use in the package for your spin axis?

MR. FRANDSEN: Generally we used the geometric center of the package unless we got a terribly distorted sine wave, in which case we started again. We tried to find the magnetic center of the package. If it was really, obviously, off-center – like a large case of electronics – and we got a grossly distorted sine wave, we started again. In most cases, we would take a gradient probe and sniff around to find the hottest part of the package, and that would be our center of rotation. There is a real problem, as was mentioned yesterday, by Jim Maclay: How do you define the criteria for the test. You say, "So many gamma at 3 ft" – well, from where? From the center of the package, or from the hottest spot, presumably? If you say that it's 3 ft from the hottest portion of the assembly, there is a problem in predicting total field, because the total field prediction is based on the center of the package and not the hottest spot.

Well, there is a problem of definition: How far from the package – or where the measurement is taken from?

MR. IUFER: At Ames we have established a policy on this. In all cases for assemblies, we use the center of gravity. To determine the geometric center may require a professional man who knows how to handle this, and rather than get into it, we use the center of gravity. For one reason, it is usually very carefully defined fairly early in the program, because this is important for spin-stabilized spacecraft.

If we do not have the documentation of this, we take a standard draftsmen's triangular scale laid on the counter, balance the box on it and find the cg by the knife-edge test ourselves, on the spot. It only takes a couple of minutes, and we can do this to a reproducibility of perhaps 1/8 in. We find that it is very important to establish some reference point in the interior of the box for all measurements.

For, even if you have a dipole source, the percentage error in field prediction is going to be three times the percentage error in the range value or measurement.

I have another question: Do you care to comment on what was the basis of your 5% accuracy? Was this magnetometer performance or an overall system error?

MR. FRANDSEN: This was primarily the limitation of method, the things that you pointed out. When I say we used the geometric center of the package, I mean just by eyeball. I mean we didn't make any particularly careful measurements to try to find the center of the package.

There were three things that I mentioned as sources of error. One, there was some problem of attaining exact perpendicularity between the three planes of rotation because of the unusual shape of some assemblies — we used little spacer blocks where necessary — not rotating about the exact magnetic center, and also because most assemblies have multiple sources. This was really just an estimate on my part. I feel that they were typically in error by ± 5%.

I can go further by saying — comparing typical packages, one with another — they vary by that amount, but that could be differences in the packages themselves. But, even in the same test, when repeated, sometimes it gave us some variance.

MR. IUFER: Were these packages depermed and exposed to standard fields at all?

MR. FRANDSEN: No, we took all of our packages after flight approval shake, possibly blindly thinking that they would all have the same type of background. This was a pretty strong shake, it wasn't as strong as a type approval shake, but it was a strong shake. This, we felt, would sort of overshadow any previous magnetic history, so we just took it from our flight approval shake and then mapped it. There wasn't any deperming.

MR. IUFER: We found that all sorts of curious things happened to these assemblies. We found that screw drivers had been used, which (when measured right at the tip of the blade) had a field of 25, 30, maybe 50 gauss. So, once it gets out of sight, almost anything can happen to its residual induction. Do you have any constraint on this so you can get data on handling?

MR. FRANDSEN: Well, no. I personally feel that we can improve the method in future programs. It left our facility and, in most cases, it was delivered right to the spacecraft assembly area where they have procedures set up so they don't use magnetic tools, they use depermed tools.

MR. IUFER: May I make one last comment? The mapping procedure used is essentially identical to the one used at Ames; however, to find total field we essentially take one azimuth rotation, find the peak, and then incline about an axis in that plane to find the peak field. Then we read the radial component of the field directly, so, instead of slide ruling, you manipulate for the most favorable orientation for peak field output.

MR. FRANDSEN: This may be a better way of doing it. The method of rotation was more of an historical way of doing it when I first entered the magnetics business and we just carried it over. We didn't have the gimbaling arrangement available at the time. We just started this way and continued. There were errors, some of the most embarrasing ones were when you get a total field that was less than one of the projections that you measured. This happened occasionally, because of the lack of perpendicularity between the planes of rotation. So, whenever you have to use a method of computation to get total field, you have more chances for error. I think you are right, the method of feeling it out and measuring the maximum total field at a distance is probably as good as any that I can think of.

MR. PARSONS: You just arrived at the statement I was hoping to hear. I agree, I think the trial and error peaking up is the quickest and most rapid way to get at the number that most people want to have as soon as they can get it.

I did want to ask about your shaker tables. Did you measure the fields above your shaker tables and do you have any feel for how large these influences were when the packages are put through vibrations?

MR. CHRISTY: The highest fields that we saw were about 35 gauss. Fortunately, these shakers have been removed. I understood that they went to the Naval Research Laboratory or maybe the Naval Ordnance Laboratory.

They have been removed from here. Through some emphasis of Dr. Gaugler and some table-pounding of others we have since acquired one "low-field" shaker to replace these. As I recall, the ambient at the shaker table is on the order of 2 gauss, I believe. I am subject to correction on that. From there, we do have the larger tables that – well, I guess the 28, 000 lb force table has a field right on the head of about 10 gauss, but we have a variety in between.

MR. PARSONS: That was the point that I was getting at, that these tables are all different, and when you survey the fields, the field will be very high right down on the table, but as you move up off of them you drop off fairly well – especially for larger packages that are fixtured onto the table. The package itself may be up where the field has fallen to a few gauss, whereas you can read as high as 50 gauss right down on the corners and around the edges. We found that it was a little bit helpful to just get the technicians to learn the fact that these fields are very high down around the corners, and we persuaded them to bring the packages in and put them down on the fixture without passing through these abnormally large fields. However, we still

decided that we would use a standard 25-gauss exposure to simulate what we thought was the average-worst case in our facility, and then deperm it afterward to get the full range of perm magnitudes that are possible to exist.

Again, we got the package after the environmental test program and, again, measured (hopefully) to get some data on stability and influences. But then, in the second case, if the perm has enlarged, we depermed it a second time before it goes on to integration.

CHAIRMAN GAUGLER: Doesn't the field also reverse on the shaker sometimes?

MR. PARSONS: Yes. You mean in surveying did we find a reversal?

CHAIRMAN GAUGLER: Yes, so one time you will find out it gives you one kind of perm, and another time it gives you another kind of perm?

MR. PARSONS: It is possible if the fixture height is different for one case than another. It should hit about the same. There is a transient problem when the machine is first turned on. You do get a tremendous pulse of DC before it settles down. There is a compensation coil on most of these shakers, but after it stabilizes it does pretty well. We have tried to get the operators to always have their machines cranked up, warmed up, before he brings the package into the area.

I was going to mention just one thing more on the distance of measurement. I am not clear as to the size of the packages that you were working with, but I wonder if your distance of measurement was anywhere close to three times the maximum magnetic envelope dimension?

MR. FRANDSEN: No, unfortunately it wasn't. The size of the typical case, on the side of the octagonal bus of Mariner, I think was about 1-1/2 ft. So the largest distance was 3 ft from this, you might say, 9-in. radius device. I guess that is three times; isn't it?

MR. PARSONS: Yes. We feel that whenever it is possible, you should have measurement distances at least three times to begin to consider the package to be a simple dipole. Furthermore, if the field being measured is large enough that we can see it, we will go six times to establish the rate of fall off with distance. If we can prove this to be inverse cubed, then we feel fairly safe in treating it as a dipole from that point on.

MR. FRANDSEN: We are hopeful when we build our next shielded room we will be able to simulate the location — I am thinking of future flight programs — of the various packages in relation to the flight probe, and measure the actual field contribution with a three-axis sensor. I hope that a 12-ft room is going to be large enough for this. There is this ever-present problem that to do this early in the program you need information on the location of the packages, and you have to have released drawings and so forth. This is sometimes hard to come by.

MR. IUFER: If I can cite one example of the flight instrument on Pioneer; measured radially along the axis of a dipole we put into the package, because of 25-gauss exposure. From the actual skin surface of the package and out to 4 ft, the length of the package in this direction was about 8 in. Even at 4 ft, when we plotted backwards, we found that we did not have an inverse cube, but the slope from about 18 in. out was inverse 2.85 power. This meant that if we were to predict the contributional field of this package to a magnetometer 6 ft away, and measure it at a range of 18 in., we would underestimate its contribution by about 14%. And, 18 in. is not quite three times the 8-in. dimension of the package. In this particular package, the sources were quite uniformly distributed about its section. So we found that to do precise work, you must get out of the immediate vicinity. For Pioneer our acceptance range is 3 ft in all cases, and none of the packages are longer than about 10 in.

MR. BERGER: Berger of Lockheed. What was the correlation between the individual measurements and the summation of these to the 30 gamma that was measured on the overall spacecraft?

MR. FRANDSEN: This I never performed myself, we got the pieces of the spacecraft piecemeal, and by the time we got the last piece to map, it was already pretty well assembled. So we were ready and prepared to map a complete spacecraft by the time we had the information to compute or predict the total field. So, I never really did the prediction. It just seemed better to go ahead and measure the spacecraft. I really can't tell you. I know Bob Christy said yesterday that they had made some rough estimates, and the rough estimates were quite a bit higher than the actual field measured.

MR. CHRISTY: That was Mariner II. On Mariner Mars we made estimates after we had mapped about 80% of the subassemblies of one complete spacecraft. After these measurements were made, we estimated the field. This, I might add, was

before we found the problem with the solar panels, but at that time and pretty consistently, for reasons I don't understand, we estimated the field at the sensor (because of the spacecraft bus) at 20 to 25 gamma. We undershot on Mariner IV. Superposition techniques on the Mariner II took us the other way, so there really is not any good correlation at this point. A half-dozen data points, a half-dozen spacecraft, essentially, is all we have to correlate from, and that doesn't produce much of a line.

MR. FRANDSEN: I would say that, by and large, the packages were surprisingly similar. The same type of a package was about the same sort of a field, I guess in the majority of cases, not always. Considering we did not deperm prior to the mapping, they were surprisingly similar.

MR. CHRISTY: I was taking some notes on the earlier conversation. I think there are some appropriate comments that should be made. It hasn't really been stated, but the fact still remains that – Mr. Iufer and I go on about this continuously, but – Mr. Iufer has the luxury of having the priority in the program. Believe you me, we envy him. Where he can say "Thou shalt not," we have to come in from the back door as a lower priority science experiment on essentially an engineering spacecraft and say, "Will you please."

So there is this significant difference. Now, this has led to a number of different facets, both in mapping and in correction and in everything else. First of all, because we were of low priority, we could not prohibit such things as the cord wood modules that Mr. Casani mentioned the other day, and the nickel ribbon. The nature of this – the configuration – if you would perm it in, say, a 25-gauss field, and then attempt to degauss it, you'll find that you are in worse shape than when you started. So you resolve this by accepting that you'll take your chances on a 10-gauss shaker and hope that everything survives.

Similarly, because of this same priority and the handling constraints on the hardware – first of all, the priority prohibiting additional funds for facilities – we were not in a position to go the gimbal route for finding maximum fields. This would have been very nice; however, in making the planar mappings as we did, we can see asymmetries in the curve that tell us whether we are dipole or some other multipolar structure. Thereby, we actually know and have a measure of the meaningfulness of our distance with respect to the package.

Now, I take from what you said earlier that your science packages fall in a variety of shapes and forms, and what not. All of our electronics packages within the spacecraft bus fall into a standard package geometry. The sketch of the module that Mr. Frandsen had on the board a while ago is typical of this assembly. An array of these will go into what we call "a bread pan" or "a bay," and again we are at a standardized size. So, in this respect, center of mass versus geometric center versus magnetic center had a little less, perhaps, significance than it would in your case.

Reverting to the shaker question again, I'd like to comment that we have had a very cooperative environmental facilities group that take every opportunity to map their shaker heads and the areas above them. Most of our shakers have a section in the center of the head that is relatively low field. Typically, it's not significantly over the Earth's field, but where we get into the strong fields is in the periphery. What's been done in many cases is to provide a shield box, I won't go into the quality of it, but provide a box-type shield to protect the hardware from these fringing fields. This has been quite successful in reducing the effects of the shaker fields.

Now, I wouldn't recommend this as a cure-all, by any means, but it has been done with some satisfaction.

Lastly, I'd like to make one statement regarding what Mr. Frandsen was talking about here in his assembly mappings. He was rather humble in all of this. Actually, over a period of about 8 to 10 mo. I'm not sure of the time, they mapped and sent reports to me of some 2,000 different assemblies. Now this does not include hardware that went up the hill, as we refer to it, that went to be mapped just because the engineer wanted to see what it looked like, and that, I imagine, would number another 1,000. When he speaks of this, be well advised that this was no small undertaking.

MR. FRANDSEN: I might say, because of the quantity, I look even more favorably up the method that Mr. Parsons and Mr. Iufer use, the method by gimbaling, because it is a lot faster. Each of these methods requires a little bit of computation with a slide rule.

MR. NORRIS: I am the cognizant engineer on the Mariner Mars magnetometer and I'm a little bit surprised to hear that people handle hardware in such a way that you would find the center of gravity. I think if we saw anyone in the lab doing that, we would probably throw him out of the door bodily. So, these are the kind of constraints that Mr. Frandsen has to work under.

MR. FRANDSEN: Well, it is true that you have to handle it very carefully, but we use wh[illegible] gloves and handle it with a lot of care. I'm sur[illegible] these peopl[illegible] [illegible]o too.

MR. IUFER: Well, I don't know if I should start off by saying that you know the structural integrity of your equipment, and we know ours. You don't use the same techniques to determine the center of gravity as you would to crack walnuts, I assure you.

I would like to make a couple of statements on behalf of the JPLers who are doing magnetic measurements. Perhaps I should wear my hat as the manager for magnetics on Pioneer to make these statements, because they are from the management point of view. That is, the people who are studying these specimens for measurement and control have a great deal of information, and we find that in some of the – well, there is an extremely good man at Goddard, Mr. Parsons, who has been working in gamma fields long before Sputnik took its spin around the earth. Perhaps I can characterize this. If you were to commission a very fine painting, you would not spend your budget on brushes and then look around for someone to fill in to use them, you would hire an artist and let him provide his own materials.

There is a great deal of experience and knowledge afoot that this field of magnetic measurements, magnetic contamination control, is a specialty. It is not a collateral duty that you can give to a skilled scientist or a skilled engineer who has had a few months of exposure to Maxwell's equations and fiddled around with transformers. It takes very practical and concrete knowledge to make good design decisions. It also takes very clear-sighted soul-searching to identify what characteristics you require of a facility.

Sometimes the important parameter is only stability, another one it may be low field. You may not need to make measurements concurrently by doing it in steps, you may cut your facility budget in half. What I am saying is that the control of a magnetic-restraints program should be given to a man who has this as his prime responsibility, because the other areas (who are being championed by individuals) usually do not work at divided purposes. So you need a fighter for reliability, you need a fighter for magnetics, you need a fighter to keep the power consumption down, and perhaps another one for the weight, and so on.

By having highly-qualified people who are very well trained in their area to discuss this, you then can come up with a good design decision.

MR. PEIZER: Peizer of the Naval Ordnance Laboratory. It appears that there has been quite a bit of data taken. I am wondering whether this has been summarized. In other words, it appears that you may be able to say that certain types of items will give you so many gamma per cubic foot, or something of this sort.

MR. CHRISTY: The answer is yes. Hopefully, some day it will all be documented, but right now we are having trouble getting into the office because of it.

MR. PARSONS: We have a summary of ours, but the trouble is, when you try to compress it into something reasonable, you drop out a lot of numbers. All I have here, for example, are peak values of the radial component on line with the moment of the box for four or five different magnetic states: initial, post-exposure, post-depermed, stray, and induced.

MR. IUFER: Pioneer also has plans to publish guidelines, parts lists, and engineering rules of thumb.

MAGNETIC TEST FACILITIES AT AMES RESEARCH CENTER AND MALIBU

E. J. Iufer
Ames Research Center
Moffett Field, California

Five years ago the only magnetic test coil facilities capable of testing a complete spacecraft were located on the East Coast. One was at the U.S. Naval Ordnance Laboratory at White Oak, Silver Springs, Maryland. And this is the one that we saw some pictures of in Dr. Beischer's talk. I believe we also saw some in Mr. Parsons's talk.

There is another large facility with a 20-ft coil system at the Fredricksburg Magnetic Observatory in Virginia.

In the spring of 1960, a triaxial Fanselau coil facility was erected in Corral Canyon, 3-1/2 mi north of Malibu Point, and located 30 mi north of the Los Angeles Airport. The primary purpose of this facility was to provide an environment in which the external magnetic fields of complete spacecraft could be precisely measured. Figure 1 shows a site plan of the crest of the hill on which the facilities are built.

Way in the background is the Pacific Ocean, on the left is a silo-appearing building with a vent in the top. This houses a three-axis 20-ft Fanselau coil. The Fanselau design is based on four loops in parallel and having their perimeters on the surface of a sphere. To the left of the silo is the so-called "Phase 2" OGO test building. This is an air-conditioned, nonmagnetic test house. The small structure up on the bank is the water supply, and the rather cubic structure in the right foreground is an approximately 40-ft cube building that was made of nonmagnetic construction to provide enclosure for tests in Earth's field.

Figure 2 is a close-up of the coil enclosure, to give you an idea of the scale. It is about 40 ft in diameter, and is of frame construction with plywood outside. After it was built it was necessary to guy it, because the wind loading was tilting the pad on which the foundation of the coils were supported.

Figure 3a gives you an idea of the size of the building, which was designed specifically for OGO tests in Earth's field. This building was also used to house the electronics and magnetometers.

Figure 3b is inside of this building. On the right is the OGO spacecraft, under the drape. In the floor is a gimbal that permits very precisely controlled

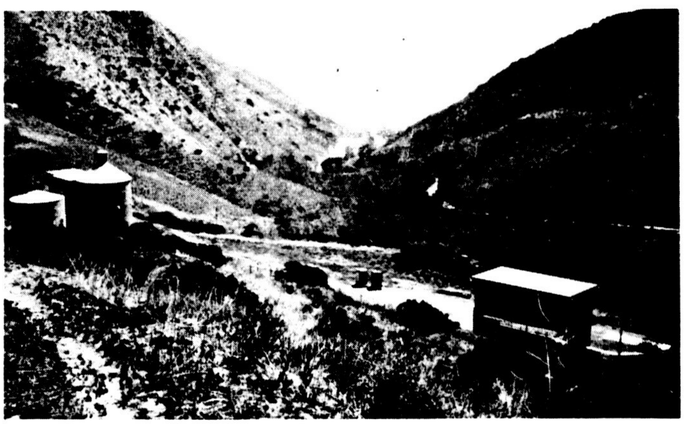


Fig. 1. NASA Malibu Magnetic Facility

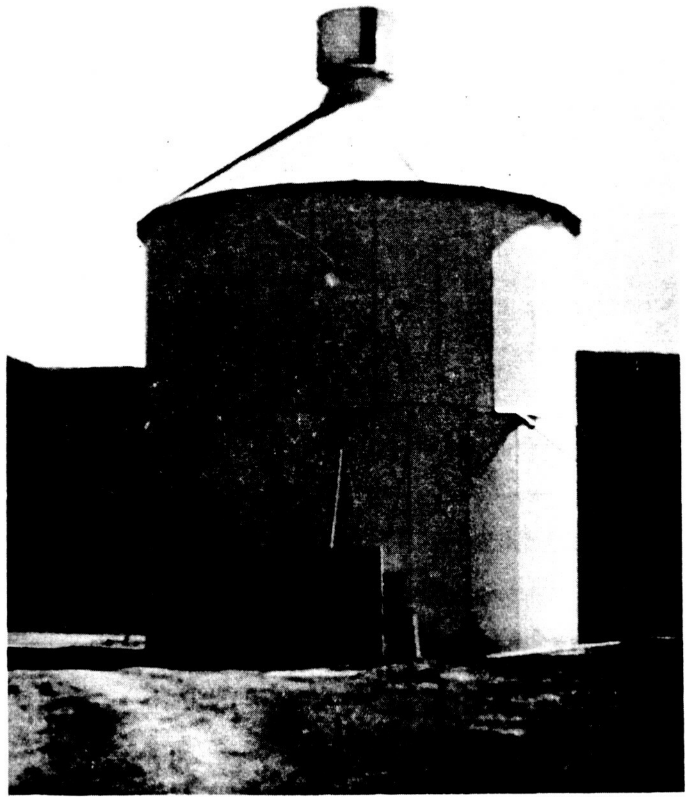
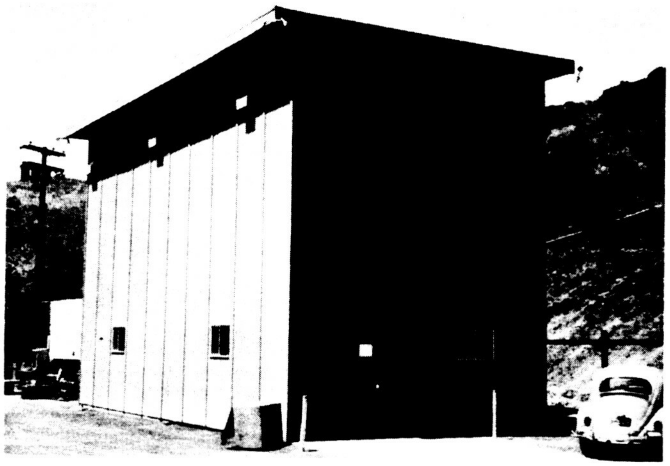
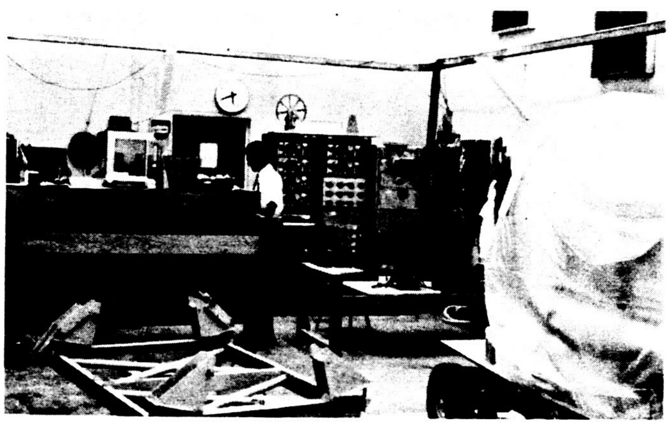


Fig. 2. Tri-axial 20-ft coil enclosure



a. Outside view



b. Inside view

Fig. 3. OGO phase I nonmagnetic building

rotation in azimuth. It has structural uprights on it with small rollers, which will receive rings that have been fixed to each end of the spacecraft ground handling equipment, so the spacecraft can be rolled about its own longitudinal axis, and it can also be rotated in azimuth.

In the background you see recorders and controls for the magnetometers. In this particular installation, the field of spacecraft is mapped in Earth's field using magnetometers at specially-determined positions, which were selected to lend themselves to evaluating the coefficients of spherical harmonic analysis without too much effort.

A computer program is then used to produce calculations of this field at any given point that was not specifically mapped.

The coil system in the silo was designed to a tolerance of approximately ±0.0005 in., using mechanical and optical techniques. The orthogonality of the coil axes are approximately 0.5 mrad. The coils are fed using stable power supplies and are capable of reducing Earth's field to zero plus or minus 1 gamma over a sphere having a diameter of approximately 3 ft.

The drift in the system is such that it can drift 1 gamma in a matter of minutes. Presently, there is no dynamic compensation because of changes in the ambient field, whether they are of geomagnetic origin or man-made.

Power consumption of the coils is such that it requires about 100 w to take care of Earth's vertical field component, and 50 w to take care of the north-south component; so that heating of the coils, because of the power supplied them, is not a factor. The coil power supplies are stable to approximately 10 parts per million. This is necessary because an error of 10 parts per million corresponds to an error in Earth's field compensation of approximately 0.5 gamma.

The noise level in the general vicinity of this site is 1 gamma, exclusive of true geomagnetic noise such as the diurnal variation or the solar storm activity.

The facility is owned and funded by NASA and is operated by the Space Technology Laboratory at Redondo Beach. The facility is in almost continuous operation in support of spacecraft contracts for the Space Technology Laboratory.

I will talk about a more modest installation at Ames. At the beginning of the Pioneer program a decision was made, by the project office, to design and build inhouse a small magnetic standards laboratory at Ames Research Center. This Center is located near the southern tip of San Francisco Bay. This laboratory is

used to screen parts, train experimenter personnel, and perform magnetic acceptance tests on each of the Pioneer scientific instruments.

The first step in the development of the program was to find a site where the magnetic noise was sufficiently low. Sites in a 25-mi radius were investigated without too much luck, because of the relatively high population in the San Francisco Bay area. The best site was about 1/2 mi north of Ames Research Center in tidelands already owned by the government. This location is approximately 1/2 mi from the nearest highway, and the only road that leads to it essentially just services this facility.

The noise at the location of the facility was measured, and the frequency spectrum or power spectrum was sampled from zero to 5000 cycles. The noise was less than 0.1 gamma, except 60 cycles and harmonic frequencies thereof. At 60 cycles, we found the greatest field, and that was 2 gamma during working hours.

We might point out that we have a severe 60-cycle, 120-cycle, and 180-cycle problem, because the connected load at Ames for the centers exceeds 100 Mw. When they operate the wind tunnels, you can expect very large fields.

The enclosure for the magnetic standards laboratory was made entirely of nonmagnetic materials, so that it could be used as a region where tests could be performed in undistorted Earth's field for low gradient measurements. Aluminum nails and brass fasteners were used throughout.

The foundation is a concrete pad weighing about 32 tons. It is 1-ft thick, and it was poured continuously without reinforcement of any kind. The walls and ceiling are insulated with 4 in. of rock wool to provide thermal lagging. If you look at the coefficient of thermal expansion for aluminum, you find that it will expand something like 30 parts per million per degree. Based on a °F this means about 1 gamma change in output when you are compensating Earth's field, per °F.

Because our tests are based on a plan where measurements can be taken over a few minutes, all we need to do is prevent rapid changes in temperature, which we do by this thermal lagging. We have a thermograph in the room, and during operations we found that the heat load furnished by the electronics in the building is such that we can maintain temperature stabilities of approximately 2°F/hr.

The building outside dimensions are 15 by 30 ft. The north wall was built as a panel so that the main coil assembly could be completely assembled and aligned in the erection shop and then transported by rubber-tired crane 1/2 mi out to the laboratory.

We wanted this facility to be ready and operating by the time the first "customer" showed up. So we designed the building, the coils, and the electronics concurrently. In a matter of about 3 days, we put the coils in, closed up the wall, wheeled in the electronics, and hung out the shingle. The main coil set is composed of 12 individual square coils approximately 12 ft on a side. The basic design could be traced to Sidney Rubin's work on five-loop square coils. These coils are arranged so that the three components of Earth's field are mutually coaxial with the coil set axes. What this means is that when they laid the forms for the pad, we had them lay the long axis of the building, parallel to the local magnetic meridian, within 3 min of arc; so that one axis of the coil could essentially take care of 99.9% of the Earth's horizontal component. The residual error we can take care of by a simple Helmholtz coil oriented in an east-west direction. The success of this was demonstrated on several occasions where we have had to change the polarity of the correction we make for the east-west effect.

It is unlikely that the building is rotating, so we are assuming that the direction of the magnetic meridian is swinging on both sides of the axis of our building. Each axis is individually driven, using the current supply stable to 0.001%. Spacing of the coils and number of turns have been designed to produce fields having a maximum volume of uniformity.

We took Mr. Rubin's design one step farther so we would get a more spherical shape volume of uniformity. His original design produced a more right circular cylindrical type volume of uniformity where the length to diameter was not unity.

As far as performance goes, the system permits an attenuation of Earth's field by a factor of 10,000 over a volume of 1 ft^3, and by a factor of 1000 over 1 yd^3. The field measurements capabilities of the lab are in the range of 1 to 30 kilogauss AC or DC ±2%, and in the range of zero to 1 gauss DC, ±0.02% ±0.2 gamma, whichever is greater.

Inside the main coil set is a high field Helmholtz pair capable of producing a 30-gauss field for perming and deperming. This coil has a diameter of 4 ft.

The advantage of rectangular coils is that they blend right into the walls of the room and provide a very free working area for manipulating samples. The test area is about 6 ft off the pad, so we had a little nonmagnetic platform to make it more convenient to operate the gimbal. The Helmholtz coil is 4 ft in diameter, and

it is oriented vertically. Coaxial with it is a gimbal system that permits rotation in azimuth and rotation in inclination.

The readout is by means of a magnetometer, which is in a little device suspended from a trolley overhead. A pipe coming down supports the magnetometer, which can be oriented radially or in the other two axes. Normally, it is positioned radially. By moving back and forth on the trolley, we can take measurements at various ranges from the sample. This shaft extends into a concrete block, weighing 300 lb, that is hollow and terminates in a 10-turn trim potentiometer which is used as a voltage divider to move the one axis of an X-Y plotter; the output of the magnetometer moves the other axis, so we have a $10 servosystem used to actually plot azimuth position versus field intensity.

Exclusive of electronics, the building and the coils cost approximately $20,000. It was built in approximately 120 days, using noncrash type funding and no overtime.

All the electronics are essentially commercial manufacture, which resulted from rather extensive market surveys. Figure 4 gives a little more detail of the gimbal.

It is a very simple gimbal. The width of the vertical member is 2 in. As I recall, it is approximately 1-in. plate. It was made of 6061 aluminum, and it cost less than $400. Because we rotate about the cg, we have counterweights and a little jack screw just under the little table in the center so we can position the sample on the gimbal and keep it in balance. So, actually we can now clamp the specimen to the table, which is made of plywood, by proper mounting hardware – we can set it by hand on this and check to see if the gimbal starts to move because of imbalanced moments. We can just move the footprint of the specimen on our table top until this imbalance ceases to exist, in which case we now are positioned so the cg is in center of balance of the gimbal. The round table will slip, and has a friction clutch at the top of the jack screw so, if we wish to incline the sample or map in any vertical plane (which would be like a great circle through a longitudinal line), we can rotate the specimen on this table until that plane of rotation is normal to the little trunion axis.

Since this photograph was taken we have quite precise setting circles on all the axes, because we found that you can't do precise work unless you can orient your sample to 1 deg or better.

All the electronics for the system are mounted in a wooden console we designed and built ourselves, and they are mounted on casters so in a very short

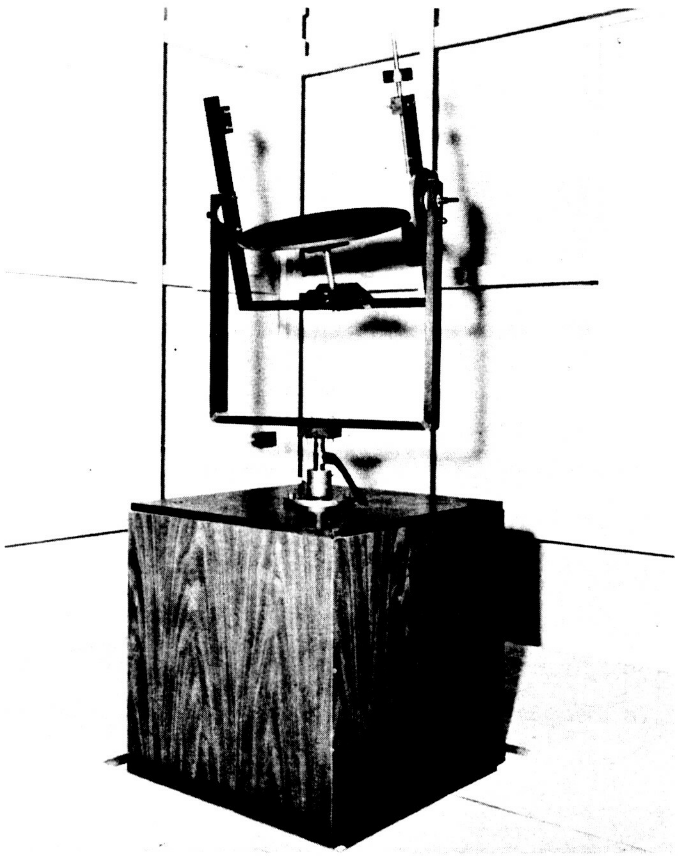


Fig. 4. Gimbal system for magnetic mapping

time we can wheel all of the magnetic material away from the building and work on extension cables. We have found that we can tolerate some 300 lb of electronics 15 ft away from the coil center, and make measurements reproducible to 0.1 gamma. If anybody moves anything, we can be off many gamma, but (as long as the material is stationary), we find that the gradient produced by a rack of electronics does not interfere with our measurements.

In tomorrow's paper, I will discuss some of the performance requirements based on the measurements for which this facility was designed.

OPEN DISCUSSION

MR. FRANDSEN: I have a question on the supporting of the nonstandard shaped assemblies, such as protrusions or experiments or something that isn't in a nice box. How do you support this by clamping without possibly damaging something or damaging a gold surface?

MR. IUFER: I seem to be coming reluctant about pointing out advantages Pioneer has. For the experiments – each one has a flat mounting surface that mounts to the instrument shelf. We use this. If someone wants a subassembly, which may be a mother board with modules on both sides of it, measured. This same individual takes off his belt and his watch and his glasses, and becomes nonmagnetic, and manipulates the sample in the test area. We don't handle it. He knows what can be stressed and what can't.

Some of the support tricks we have used have been masking tape, blocks of wood, ordinary molding clay, scotch tape (with these sort of things we were able to obtain the stabilities necessary) to actually anchor it in position.

MR. FRANDSEN: I wonder if Mr. Parsons could possibly say something about the way they clamp theirs. This really has been a bugaboo to us because of the gold surfaces and odd shapes.

MR. PARSONS: We use a gimbal very similar to Mr. Iufer's here, except it is a little heavier. As a matter of fact, we have two; one is designed for the smaller packages of the type we got from the IMP spacecraft program, and the other is a larger set of identical equipment for the bigger components. The IMP package is roughly 9 in., it is the number limit for its dimension; for the OGO, we can go up to

16 or 18 in. What we have is a pair of plates inside our gimbal box that are adjustable toward each other by a system of lead screws. These are padded with a 1/4 in. rubber pad. We bring the package in; the experimenter, or whoever is responsible for it, is always with it. We show him how we plan to mount, and he decides "yea" or "nay" on the mechanical stressing of this thing.

Normally, we have had no problem on this at all, and the point was raised back there before, so I am trying to be a little cautious – but actually we haven't had any trouble on this. We put the package in the concentric circles inscribed on these rubber pads, place it in there, and clamp it down. The man who clamps, of course, is experienced in this. He doesn't mash the thing flat, but he gets enough grip on it so it won't fall out. The only hazard that we ever had was in the early days when we were a little too cautious with one package; we didn't squeeze it quite tight enough, and it did fall out. This does disturb the experimenter no end.

Since that time, we have taken several precautions. We have foam padding on the floor below. We have restraining elastic bands on our boxes now; so, even if the package would come loose, it would slide only to the edge of the shelf and then be restrained by elastic bands. This takes care of 90% of our problems. Occasionally we get an oddball device that won't lend itself to this kind of mounting. So, then you have to build a little fixture especially for that purpose. We have several of these now that are made of plywood sections, etc. with nonmagnetic bolts, etc. We can mount almost anything, but to do it most rapidly and conveniently, the squeeze method has worked the best for us.

MR. BROOK: We are now having a problem with one of our experimenters. I believe he is from Goddard and there is a problem of magnetic materials. The question of an oddball shape. You handle 90%; well, I have the other 10%.

It is something like a 100 in. X-ray telescope, which may or may not be made of magnetic materials. These are being investigated now. I think that Mr. Parsons will probably be stuck with the problem of checking it out.

Do you care to comment on that?

MR. PARSONS: Well, we take on all comers. We don't guarantee results, but I have looked over one preliminary early model that I think, of a similar scope, was associated with one of the programs. It is going to be a problem, all right. So that is what we live on – problems. We will fixture it as necessary. Hopefully, by the time this thing gets to us, we may have some of our newer facilities ready. We

will have a larger volume in which to work and move. We will have some more sophisticated fixturing, which we are trying to get designed now. We will cope with the problem.

MR. IUFER: On this telescope, you might consider having a magnetic specification on the transit case and just leaving it in it.

MR. CHRISTY: Just a comment on that. We have done this in some cases on Mariner Mars. The handling frames were made out of A-286 and titanium and a few cold-rolled bolts. But we replaced the hard steel stuff, particularly for the communications packages where they are extremely fussy, and rightly so. We did use this sort of technique and it is quite satisfactory.

MR. IUFER: Be sure and have the man demonstrate that; it is really in the transit case when you measure it.

STABLE FIELD ENVIRONMENT FOR MAGNETOMETER TESTING

R. E. Brown
U. S. Naval Ordnance Laboratory
White Oak, Silver Spring, Maryland

The Naval Ordnance Laboratory, White Oak, under NASA Headquarters sponsorship, is working to improve the performance of fluxgate magnetometers to be used in space magnetometry. It consists of analysis of the factors influencing the sensitivity, stability, noise level, etc., of fluxgate type magnetometers. To evaluate these factors in the gamma range, it was considered necessary to design and construct a low magnetic field region where extraneous fields and gradients would be held to a minimum. The requirements to be met by this facility were derived from NASA specifications set forth to be the goal of future space magnetometer development. The requirements thus derived for the test space may be divided in two categories: (1) certain limitations on magnetic field fluctuations and (2) limitations on the total magnetic field level. For the first category, the low inherent noise level desired of magnetometers suggests the need for a test space with a peak-to-peak noise level less than 1 milligamma. In addition, the freedom from zero drift required of magnetometers used in space exploration suggests the need for a minimum long term field variation in the test space below the 1 milligamma per day figure. For the second category, it was necessary that the total field level in the test space be reduced to the order of the fields to be measured in space, that is, a few gamma. It was desired, if possible, that the total field be reduced well below 1 gamma to enable testing sensitivity and performance of magnetometers near their inherent noise level.

It appeared that the field stability requirements for the test space would be the hardest to meet. Achievement of such field stability seemed possible to us only through the use of superconducting magnetic shielding. The use of a superconducting enclosure in this way to attenuate magnetic noise, although known for many years, may not be familiar to everyone. How such an enclosure compensates for a magnetic disturbance may be understood because superconductors possess zero electrical resistance. Imagine a perfectly conducting hollow cylinder placed axially in a solenoid. When current is turned on in the solenoid, a magnetic field propagates inward from the windings. When the field reaches the cylinder walls, eddy currents are set up in a direction to cancel out the local change in field. If the material of the cylinder did not have zero resistivity, the eddy currents would dissipate as heat and

the magnetic field would enter the hollow cylinder after a momentary retardation. If the hollow cylinder is made from a superconducting material, such as lead cooled to $4.2^{\circ}K$, the eddy currents persist and the magnetic disturbance created by turning on the solenoid never reaches the interior. Data will be presented later to show how successful this persistent eddy current mechanism can be in preventing changes in ambient field from reaching the interior of a superconducting cylinder.

To meet the second category of requirements, the reduction of the total field in the test space to the level of a few gamma, we have been using a dynamically balanced three axis Helmholtz system. Thus, it was planned that the full achievement of the test space specifications would be carried out in two stages. The required low field would be established by the Helmholtz system. Then a hollow enclosure made from lead would be placed in the low field region and cooled until the lead became superconducting. Any subsequent changes in the magnetic field outside the superconducting enclosure would be greatly attenuated, before reaching its interior, by the persistent eddy current mechanism. The low field, stable field region inside the lead enclosure would constitute the magnetometer test space. Room temperature access to this space would be made possible by inserting the tail of a dewar vessel into the hollow enclosure.

The major part of this report is concerned with the performance of superconducting shields. The performance of the Helmholtz system will be dealt with only briefly. This system is a commercial unit built by Vickers. The coils are approximately 8 ft in diameter. A three axis magnetometer within the coil system senses field and continuously signals to the control unit for changes in the coil currents. The control system has a response time of approximately 1/10 sec. Field fluctuations of longer duration than this response time are kept to about ± 2 gamma. Thus, a minimum 2 gamma field may be established at a point within the coil array. The system does not correct for field gradients nor does it eliminate the rather prevalent 60 cycle field.

EXPERIMENTAL ARRANGEMENT

To evaluate the basic shielding properties of a superconducting enclosure, a series of field attenuation measurements were made on a particular shield. This shield was an open-ended hollow cylinder 10 in. long by 1-5/8 in. in diameter. The cylinder was made by rolling up a sheet of 1.5 mil lead foil and joining the edges

together by careful melting. The lead was of technical grade of no better than 99% purity. The cylinder was fit snugly around a single vacuum jacketed glass dewar that provided a 1-in. diameter room temperature access to the interior of the shield. This assembly was inserted into a stainless steel liquid helium dewar for cool-down.

For this series of attenuation measurements, no use was made of the Vickers Helmholtz system. The shield was cooled to $4.2^{o}K$ in the presence of ambient Earth's field. When a shield of the type described becomes superconducting under these conditions it will "freeze in" a modified version of the ambient field distribution in its interior. The modification is the result of nonuniform cooling and of a partial Meissner effect in the lead foil. Some of the magnetic flux threading the walls of the cylinder is expelled spontaneously and some remains trapped. The "frozen in" field distribution will normally be invariant with time following cool-down.

The attenuation measurements were made by noting the signal from a magnetometer at some point within the shield when a test field was turned on outside the shield. The test field was supplied by a Helmholtz pair 3-ft in diameter and was uniform to within 5% over the volume of the shield. The magnitude of the test field was 5 to 10 oersteds. A Hall probe was used to measure the higher fields near the open ends of the shield and a fluxgate magnetometer was used for the lower level measurements. Measurements of 60 cycle field attenuation were made by substituting a 10,000 turn pickup coil for the magnetometer.

RESULTS

Measurements were made for two orientations of the test field and magnetometer sensors. Figure 1 shows the results obtained with both the test field and magnetometer sensor oriented transverse to the axis of the cylindrical shield. In the figure, H_x is the field appearing at some point along the shield axis when the test field of magnitude H_o is turned on. The ratio H_x/H_o is plotted vertically on a logarithmic scale. The distance, in inches, of the magnetometer sensor from the top end of the shield is plotted horizontally.

The attenuation appears to follow a very simple linear logarithmic law. We have made no attempt to compute the response of a superconducting shield to magnetic field, but this law must result from the manner in which field finds its way into the interior of the cylinder. The test field is not allowed to penetrate the walls of the cylinder significantly beyond the thickness of a small multiple of the London

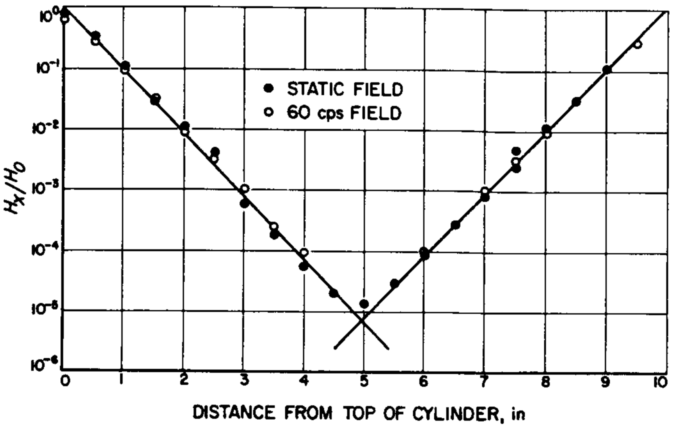


Fig. 1. Attenuation of transverse field

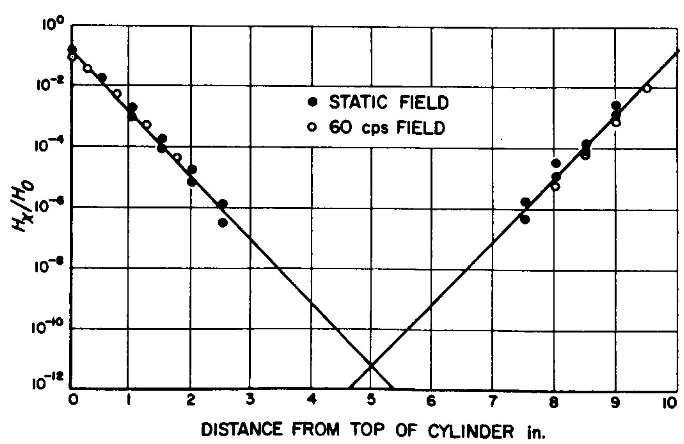


Fig. 2. Attenuation of axial field

penetration depth of about 10^{-5} cm. The amount of field that leaks into the shield comes through the open ends and must be determined solely by the field distribution there.

It may be seen that one finds the same attenuation for either static or 60 cycle fields. After reaching a minimum of 10^{-5} at the midpoint, the value of H_x/H_o starts to increase again, of course, on approaching the bottom opening. If the bottom end of the cylinder were capped with lead, one could presumably expect a value of H_x/H_o near 10^{-10} at the capped end.

Figure 2 shows the results obtained with both the test field and magnetometer sensor oriented parallel with the axis of the cylindrical shield. No measurements could be made near the midpoint of the shield although the value of 10^{-11} obtained for H_x/H_o by extrapolation seems to be well substantiated.

DISCUSSION

The ability of a superconducting enclosure to attenuate magnetic disturbances, as evidenced by the above measurements, seems to be unequaled by any other shielding method. We have made no measurements, thus far, of the long term stability of the field distribution "frozen in" the shield. The axial field sustained by supercurrents around the cylinder may be expected to be as stable as the superconducting state itself, that is, essentially invariant with time. Problems may arise with the flux left threading the walls of the cylinder after cool-down because of an imperfect Meissner effect. This distribution of flux is presumably stabilized by a defect structure in the material of the walls. It is known, however, that the trapped flux may move about. Mechanical vibration, for example, may bring about a redistribution. For the best long term field stability it may prove advisable to subject the shield to severe mechanical vibration after cool-down to bring the trapped flux distribution to its most stable arrangement.

Having established the excellent shielding characteristics of a superconducting hollow cylinder we now wish to introduce it into the controlled field environment of the dynamically balanced Helmholtz system. Presumably, a number of advantages will result from this combination. A low field may be established within the Helmholtz system. If the shield is made superconducting while in this low field, a sample of low field that is subsequently invariant with time will be trapped within the shield. Any trouble with moveable trapped flux in the walls of the shield will be

minimized because very little flux threaded the walls during cool-down. The separate abilities of the Helmholtz system and shield to attenuate magnetic disturbances should be additive in the combined system.

There are a number of reasons why the situation is not this favorable or simple. The problem lies in the inability to establish the required low field over the whole volume of the shield prior to cool-down. Field gradients of considerable magnitude exist within the Helmholtz system. Stainless steel helium dewars always seem to have permanent magnetic fields associated with them that are difficult to compensate. Nonuniform cooling of the shield produces distortions in this complex field pattern that are hard to predict. At times the shield may trap, on cool-down, a momentary field fluctuation that the Helmholtz system is too slow to compensate. The best field distributions that have been trapped in the shield are less favorable than desired. Axial fields of the order of 10 gamma with gradients of a few gamma per inch over a 2-in. length have been trapped in the shield, but even this much success is never certain.

It should be possible, with a sufficiently elaborate gradient correcting coil system, to obtain a satisfactory field pattern within the shield before cool-down. Such extreme measures may not be necessary however. We now have under investigation a system of "flux pumping" that should lower the minimum field of the Helmholtz system and provide a more homogeneous field distribution within the shield.

FLUX PUMPING

Several methods have been described for lowering the magnetic field in a superconducting enclosure by using the Meissner effect in some way. Because most of these devices involve a superconducting piston and cylinder arrangement, they have been called "flux pumps." The basic principle of operation is: if a piece of superconductor is cooled in a magnetic field below its transition temperature all of the magnetic field inside the specimen is expelled spontaneously if the Meissner effect is complete. One may, in principle, obtain access to this field free region if the piece of superconductor is composed of a hollow cylinder and a close fitting piston. Removing the piston allows access to the field free interior of the hollow cylinder. In practice, such flux pumps usually produce a field reduction of less than a factor of 20, primarily because of problems with the closeness of fit of the piston and cylinder. In addition, the Meissner effect is rarely complete unless the

superconducting specimen is of high purity and free from strain. These conditions are difficult to maintain in a working device. To solve our immediate problem, a flux pump was required that would operate at a starting field of a few gamma. Although there seems to be little work on the completeness of the Meissner effect at these field levels, it was suspected that flux pumps depending on this effect would become less efficient with decreasing starting fields.

FLUX EXPULSION

It is possible to imagine another way in which a relatively field free region might be produced inside a piece of superconductor without depending on the Meissner effect or on the piston-cylinder arrangement. Imagine a slender rod first brought into the superconducting state and then pierced lengthwise to produce a slender hollow cylinder. If the diameter of the hole in the cylinder could now be greatly increased to produce a thin-walled hollow cylinder of large diameter, the field inside would be much reduced below the ambient field. The ambient field would be pushed aside by the advancing walls of the expanding cylinder because of the shielding currents set up in the superconducting material.

It does not seem feasible to carry out the formation of a thin-walled hollow cylinder in just this way. However, a hollow cylinder made from lead foil may be folded into a compact form while in the normal state and then reopened while superconducting to approximately the overall operation. Figure 3 schematically illustrates the stages in the operation of this flux pump. We have chosen to simply flatten the lead foil cylinder, as shown in Fig. 3b, although more compact folding should be feasible. The flattened cylinder would be pleated and folded or rolled into a compact rod shape. In the simple flattened form no reduction in the field component perpendicular to the flat side of the collapsed cylinder can be expected on reopening (Fig. 3c). The axial component of the field (illustrated in the figure) and the transverse component lying in the plane of the flattened cylinder should be reduced by a factor equal to the ratio of the closed cross sectional area to the opened cross sectional area of the cylinder.

A model of this flux pump has been constructed and exhibits an axial field reduction ratio of 30. This ratio is constant for starting fields ranging from 10 gauss to a few milligauss. If the cylinder were flattened and clamped tightly between rigid plates during cool-down, a field reduction ratio in excess of 100 should be easy to attain. Although the field reducing property of the flux pump has been emphasized,

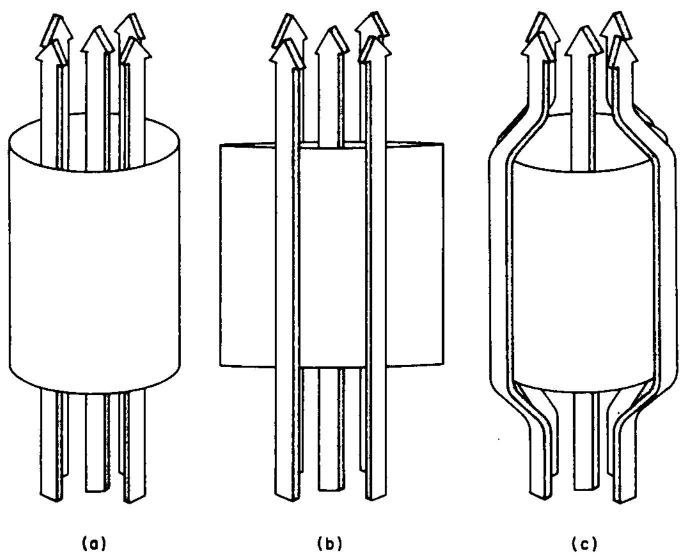


Fig. 3. Stages in operation of flux pumps

(a) Before collapsing
(b) Collapsed
(c) Reopened

it should be clear that any uniform reduction in field brings about a reduction in field gradients as well. Thus, the lowered field inside the opened cylinder is also a more homogeneous field.

CASCADED OPERATION

Cascaded operation of this pump should be possible at the expense of increasing complexity of the opening mechanism. After one cylinder is opened, another folded cylinder could be lowered into it. This second cylinder would need to be surrounded by a heating device to keep it in the normal state until it was in position in the lowered field inside the first cylinder. The second cylinder must be allowed to become superconducting in this lowered field to achieve the cascading effect. If properly oriented, the second cylinder would help correct the deficiency of the first cylinder in reducing one of the transverse field components. The extent to which cascading may be carried out in practice will be explored in future models of the flux pump.

SUMMARY

Our purpose has been to investigate the use of superconducting shielding in the construction of a low field, stable field magnetometer test facility. Our findings thus far may be summarized as: a superconducting enclosure possesses the ability to attenuate magnetic disturbances unequaled by any other shielding method. We believe that our flux pump, used in conjunction with a dynamically balanced Helmholtz system, will make it possible to attain very low homogeneous fields in such an enclosure. Field levels below 0.1 gamma should be possible. A prediction of field homogeneity cannot be made at this time with any degree of certainty.

OPEN DISCUSSION

CHAIRMAN GAUGLER: I think Dr. Brown is rather conservative when he says he has a noisy environment. It is next to a rolling mill, a couple of swedging machines, and it is about as noisy as any factory could be.

MR. PARSONS: I don't think I understood you to say what sort of detector you were using to see this 10^{-4} gamma. Is that what you said? How are you sensing this?

DR. BROWN: You are reading the abstra ·, I take it. You don't measure th :; as I say we simply made this linear logarit ic extrapolation. We are sure that is about what the noise level is, but, of course, it is beyond measurement.

MR. PARSONS: What sort of sensors did you operate with? You did have a flux gate?

DR. BROWN: Yes, a flux gate. It was one of our own. It wasn't anything very fancy. It had about a 1 gamma threshold.

MR. PARSONS: And what is your most optimistic dream of volume size to come from this?

DR. BROWN: Cryogenic space is quite expensive, and we are thinking of no larger than a 4-in. diameter room temperature access. If you want to know the length, it would probably be 20 to 30 in. long. We hope to use at least a 4-in. length of it. What we want to do is be able to flip over an average sensor, so we need about a 4-in. spherical volume.

MR. PARSONS: I guess it will be a while before we can bring a spacecraft inside. One other question: I have heard rumors of room temperature superconducting material. Is there any hope for that?

DR. BROWN: Just rumors.

MR. GOLDSTEIN: Would you care to comment on some of the mechanical problems involved in expanding and collapsing this shield at cryogenic temperatures?

DR. BROWN: Well, it is easy to open one of them. I haven't yet tried it in this cascading operation. It always has to be done on the end of a manipulating mechanism of course, and it is going to get more complicated when we try cascading. But, as far as opening one of them, it is no problem at all. As I say, we use 1-1/2 mil lead foil. This bends very easily — easier than I expected. You can probably use 3-mil lead foil, but 5-mil might be too thick. You might get cracking after a while. We just collapsed the thing, and we are going to open it not with a round cross-section, but with a square cross-section. You glue a couple of panels on the side and swing the panels to the side, and that opens the thing. It is really much easier than I thought it would be.

MR. CANTOR: What king of temperature for the lead are we talking about?

DR. BROWN: Oh, this is 4.2°K, liquid helium. Of course, we have room temperature access to the space, and that is, well, room temperature. We haven't really controlled the temperature. We blow warmed gas into the space, and it is around room temperature.

MR. NOBLES: Did you say that if you freeze in the ambient field it also freezes in the gradient?

DR. BROWN: Generally, yes. There is probably some modification in the field distribution when this shield becomes superconducting. It is hard to predict just what will happen. Some of the flux will remain threading the walls, and some of it will be expelled.

If there were a perfect Meissner effect it would all be expelled, but you never really see this. Particularly in this hollow enclosure geometry, it is hard for a thin piece of superconductor to expel all of the flux that is perpendicular to it. It is energetically unfavorable for it to get out, and there are little trapping centers that tend to stabilize flux once it is caught in the act of getting out.

DESIGN OF SPACECRAFT DEGAUSSING COILS

N66-11290

A. M. A. Frandsen
Jet Propulsion Laboratory
Pasadena, California

MR. FRANDSEN: I find that my title has been incorrect, according to the terminology discussed here. It should be "Design of Spacecraft Deperming Coils." So those of you who are disappointed can leave now.

INTRODUCTION

Late in the Mariner Mars program the need arose for a large set of degaussing coils. This was mainly because the solar panels were quite magnetic. A set of four nondegaussed panels would have contributed 100 gamma to the flight magnetometer reading. The contribution from the rest of the spacecraft was on the order of 30 gamma. The degaussing coils were to be capable of holding a completely assembled Mariner-less solar panels. The solar panels would be degaussed separately. The criterion of 80 gauss peak field was set, with ring down capability to 0.1 gauss. To prevent large circulating currents from being induced in low impedance spacecraft circuitry, it was decided to use a degaussing frequency on the order of 1 cps or lower.* In addition, surge suppression networks were planned to protect the spacecraft from such things as power line failure, coil windings opening up, etc.

High voltage capacitor discharge was considered as a method for producing the ring down waveform. It is attractive if the weight of the coils is to be a deciding

*This was determined by the following reasoning: A design goal of 1 gauss/amp was set, based on the capability of available power supplies. The effective area of the largest low impedance spacecraft signal path was estimated at 1 m^2 of 20 gauge copper wire. The maximum induced current would then be given by:

$$\text{(i)}_{\text{maximum induced}} = 8 \times 10^{-4} \; (di/dt)_{\text{maximum driving}}$$

If the maximum driving current is 100 amp at 1 cps, the maximum induced current is 500 ma.

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factor. One can get more gauss per pound of wire by using a high driving voltage. However, there is an inherent safety problem in using the capacitor discharge method — both to personnel and to the spacecraft. In addition, the degaussing frequency and duration are not directly controllable. Rather they are a function solely of the circuit parameters. Therefore, it was decided to use low voltage high current DC power supplies that were programmable from full power down to zero. A pair of 36 v DC, 100 amp units were selected. The bipolar nature of a ring down waveform requires that a polarity reversing relay of high current capability be placed between the power supplies and the coils. The low driving voltage dictated that the coils be low impedance for 100 amp to flow. Large, low resistance coils inherently have a long L/R time constant. This necessitates a long degaussing sequence if the ring down is to be performed in fine increments. However, that was not considered to be a serious disadvantage. Space Technology Laboratories had previously been successful in using low voltage, programmable DC power supplies in degaussing the OGO main body. This gave us added confidence in the soundness of the low voltage method.

GENERAL CONSTRAINTS

To keep the coil impedance low, the size of the degaussing coils was kept as small as possible. In addition, many parallel windings of large gauge wire were used.

There was a spacecraft handling constraint that the longest spacecraft dimension (Z-Z axis) remain vertical for all degaussing sequences. Therefore, to degauss along all three spacecraft axes, it was decided to build a pair of coils having the following dimensional constraints:

1. Inner diameter of a coil form was to be large enough to permit the coils to be lowered over the spacecraft and surround the octagonal main body. It was decided that one coil should be sufficient for degaussing along this, the Z-Z axis, because the main body will lie in the plane of the coil, at its center.
2. When the coils are set upright for degaussing along the spacecraft X-X or Y-Y axes, the separation between the pair was to be one coil diameter. This constraint is a reflection that the coil size must be kept to a minimum. With these things in mind, the coil dimensions shown in Fig. 1 were established.

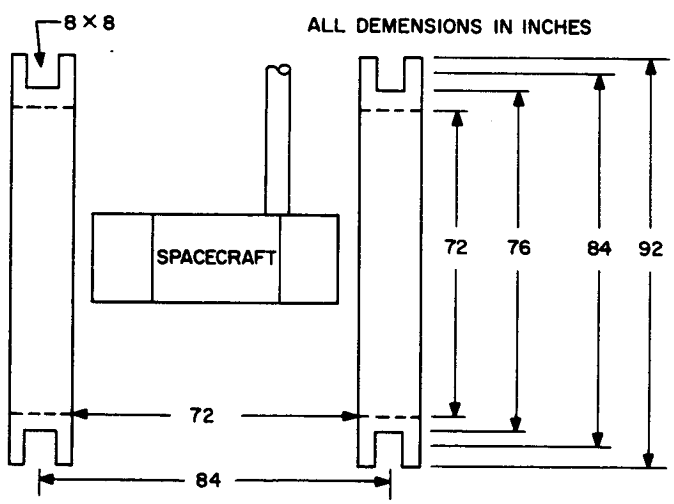

Fig. 1. Approximate dimensions of coil forms

ESTABLISHING CIRCUIT PARAMETERS

Figure 1 served as a starting point for determining the coils' electrical characteristics. At the center of the pair the field is given by:

$$B = \frac{\mu_o}{2\sqrt{2}} \frac{NI}{r} \qquad \text{meter-kilogram-second} \tag{1}$$

Where

NI = the TOTAL ampere-turns of EACH coil.

As stated, each coil was to be wound with a number, n, parallel windings to keep the coil resistance low. So, for the proposed coils, Eq. (1) can be rewritten:

$$B_{gauss} = 416 \times 10^{-5} \sum_{1}^{n} N_j I_j \tag{2}$$

Where

$N_j I_j$ = the ampere-turns of the j^{th} parallel leg.

At this point one has to decide on a relationship between the ampere-turns of the various parallel windings. If the same number of turns is chosen for each leg, then the innermost winding will carry more current and, therefore, generate more heat than the outer windings. Because this could be a problem, it was decided to keep the current in each leg the same by making them all have equal resistance. It follows that the outer-most windings will have fewer turns than the inner-most. For design purposes, an average value of the turns per parallel leg was used. Equation (2) was then written:

$$B_{gauss} = 416 \times 10^{-5} \, n(N_{ave} I_j)$$

But

$$I_j = \frac{I_{Total}}{n}$$

So

$$B_{gauss} = 416 \times 10^{-5} N_{ave} I_T \tag{3}$$

For a pair of 100 amp power supplies to produce 100 gauss requires that the overall coil constant be 1 gauss/amp. Substituting this into Eq. (3) gives:

$$N_{ave} = 240 \text{ turns}$$

The question now is how many parallel windings to use. For a given coil geometry one can show that the gauss per volt of drive is directly proportional to the number of parallel windings and inversely proportional to the ohms per foot of wire.

$$\frac{\text{gauss}}{\text{v}} \sim \frac{n}{(\text{ohm/ft})} \tag{4}$$

This says that it is desirable to have a large number of parallel windings wound with wire of large cross sectional area. Number 9 square copper wire was chosen because of its size and availability. There are, of course, practical limitations to the number of parallel windings one can get into a single coil form. However, this number is restricted by the type of power supply chosen. As mentioned, a pair of 36 v, 100 amp programmable DC power supplies were picked. With the power supplies seriesed and the coils seriesed, the total resistance must be less than 0.72 ohm. Neoprene jacketed 4/0 cable was chosen for connecting the power supplies to the coils. At this point a little inspired guessing is helpful. It was decided to allow 25% of the 0.72 ohm for hook-up resistance. The coil resistance would make up the other 75%. That is, each coil was to be about 0.27 ohm. Because number 9 square copper wire was chosen and the average number of turns per winding was found to be 240, this makes the resistance of each winding 3.34 ohms. The ratio of this resistance to the desired value for each coil (rounded off to the nearest whole number) gives n, the number of

parallel windings per coil. Therefore, for this design, n is 12. Figure 2 summarizes schematically the electrical configuration of the coils.

The coil inductance can now be estimated. This will have an important affect on the frequency and duration of the degaussing sequence and also on the design of surge suppression networks.

The average 240 turn winding consists of four layers of wire, 60 turns per layer, in an 8-in. wide channel. Engineering handbooks generally give inductance formulas for single layer solenoids rather than multilayer coils. Therefore, the inductance of the parallel windings was estimated by treating each as four, 8-in. long, single layer solenoids in series. The mutual coupling was assumed to be 100%. This gave 200 mh/leg. Twelve such legs placed in parallel, with perfect coupling, will also give 200 mh. The total inductance for the pair of degaussing coils would, therefore, be 400 mh plus 2M, where M is the mutual inductance between them. If 25% coupling is assumed, M is 50 mh. L_{Total} is therefore 500 mh. Because of the uncertainties in making this estimate, a liberal tolerance of ±100 mh was assigned. Because the total resistance of the system was set at 0.72 ohm, theL/R time constant becomes 0.5 h/0.72 ohm or 0.7 sec.

ESTABLISHING THE DEGAUSSING SEQUENCE

The long time constant of the coil system limits the maximum rate of change of current to something like 150 amp/sec. This occurs when a 72 v DC square wave is applied to the coils. It was previously established that the degaussing frequency would be limited to 1 cps, or less, to protect the integrity of the spacecraft. A 100 amp, 1 cps sine wave exhibits a maximum di/dt on the order of 600 amp/sec. It is, therefore, safe to program the degaussing sequence using voltage square waves. This has the advantage of minimizing the time required for the current to build up to the desired value. The current will reach 98% of its final value in about four time constants. Because the system time constant has been estimated at 0.7 sec, the voltage square wave should be applied to the coils for 3 sec. At this point, zero volts can be programmed and the current allowed to decay. When the current has essentially reached zero, the polarity reversing power relay must be switched. Ten time constants (7 sec) were allowed for the current decay and switching operation. This establishes the degaussing frequency at 0.05 cps or 20 sec/cycle. Figure 3 shows the field waveform produced at the center of the coil pair.

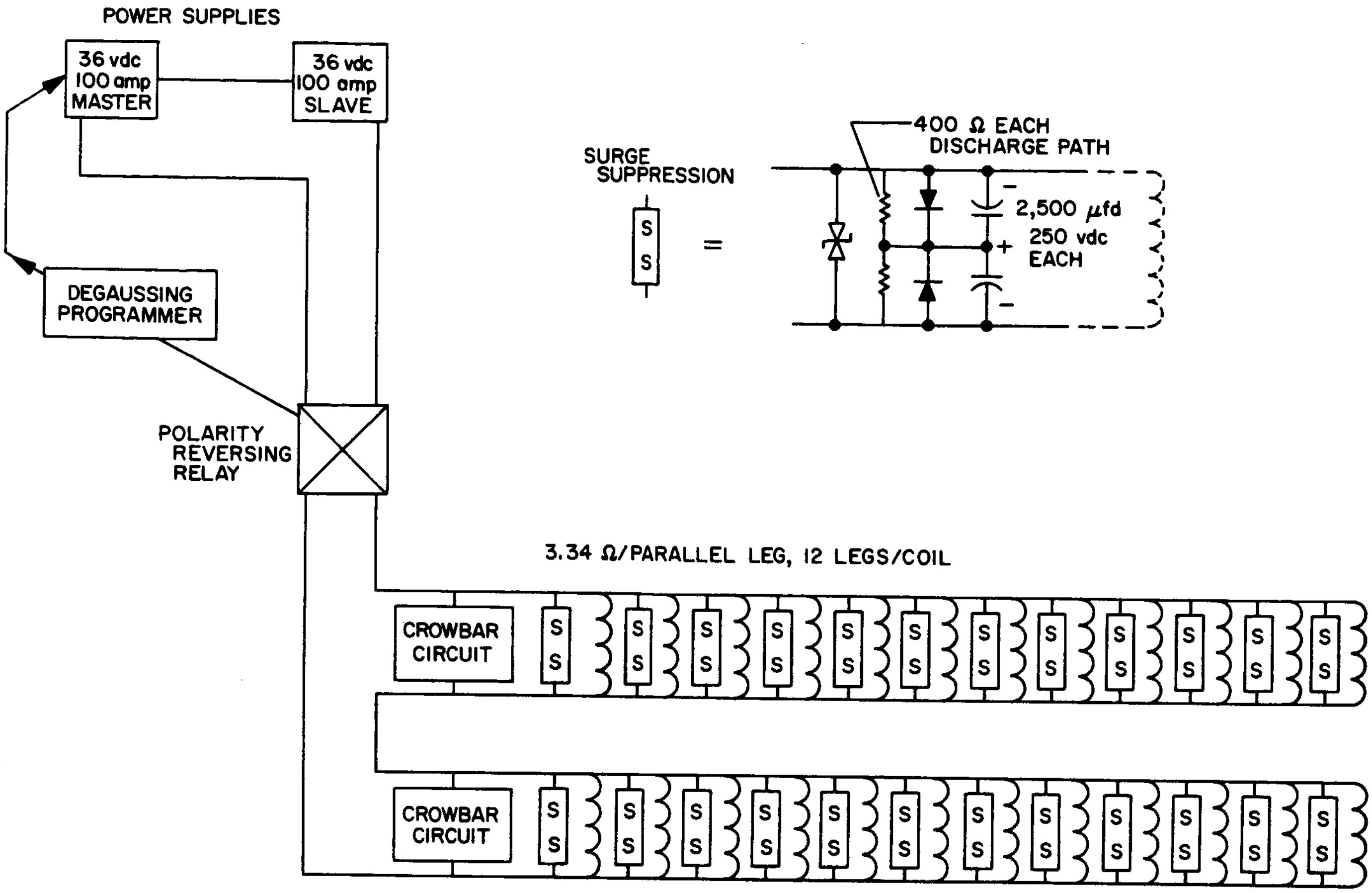

Fig. 2. Degaussing system block diagram

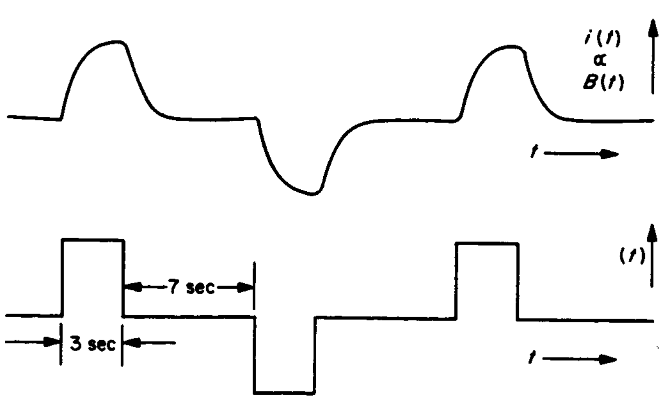

Fig. 3. Coil current, field, and driving voltage wafeforms

The duration of a complete degaussing sequence can now be determined. The st ring down that was planned called for the amplitude of each 1/2 cycle to be 1% less than that of the previous 1/2 cycle. This is a logarithmic decrement of 0.02. One can then show that the maximum time required to go from 100 gauss to something less than 0.1 gauss is about 2 hr. If a more coarse decrement is used, the overall time is decreased proportionally. If a lower starting amplitude is used, the overall time decreases logarithmically.

SURGE SUPPRESSION

The interwinding capacity of each coil was estimated at a few tenths μf. This was based on comparison with a smaller existing coil of similar proportions. The natural coil resonance was, therefore, expected to be on the order of 1 kc. If one of the power supply connections should open up during a degaussing sequence, the high frequency ringing might damage the spacecraft. So it was decided to put a large amount of capacity in parallel with each winding. This was to be done with a minimum number of connections so that each winding and its shunt capacity would form an integral unit to the extent that this was practical. For this purpose, twenty-four computer grade, electrolytic capacitors were mounted on each coil form — two per leg. The value chosen was 2500 μf. The self resonance of each coil was thereby reduced to about 2 cps. This does not quite meet the 1 cps requirement previously set. However, it seemed to be a reasonable compromise, because lowering the resonance from 2 cps to 1 cps requires four times as much capacity. Capacitors four times larger than those selected were not immediately available in the quantity required. In addition, their voltage rating was marginal for this application.

How will a bank of large capacitors affect the normal operation of the coils? It is true that the capacitors will initially shunt current away from the coils. However, the RC time constant involved is quite small; primarily because the only resistances involved are those of the large gauge cables connecting the coils and capacitor banks to the low impedance power supplies. The capacitors, therefore, charge up in a few milliseconds and have no significant effect on the 20 sec/cycle degaussing waveform.

To protect against the high voltage arcing that might occur if a winding opens up, selenium surge suppressors were connected across each leg. The devices chosen have an 80 amp current rating and clamp the maximum voltage across any

winding at ±75 v DC. As a backup to these devices, a crowbar circuit was placed in parallel with each of the two coils.

PROGRAMMING THE DEGAUSSING SEQUENCE

The power supplies can be remotely controlled using either voltage or resistance. It was felt that resistance programming would be the easiest to implement. The first programmer built consists of a 10 turn, motor driven potentiometer. The speed of the driving motor is a function of the applied voltage. As the motor turns, it adjusts its own driving voltage downward. So, the more it turns, the slower it goes. The resistance of the programming potentiometer is, therefore, an exponentially decaying function of time. A cyclic timer and cam switch arrangement is used to sample this resistance twice each cycle. Additional timers and cam switches are used to synchronize the operation of the polarity reversing power relay. The starting resistance and the decay rate are adjustable.

A second programmer was built to have redundant capability. In this programmer, fixed resistors are selected in a binary sequence by stepping switches. Time delay relay logic is used to pulse the stepping switches and operate the polarity reversing relay.

GENERAL COMMENTS

The design of the coil forms, supporting structures and handling frames was performed by Mr. Robert M. Norman of JPL. The coil winding operation was also under his very capable direction.

Each coil contains about 12 mi. of wire and weighs over 4000 lb. For spacecraft degaussing, the maximum power dissipated in the coil is on the order of 1 w/lb of copper. However, in anticipation of possible future applications, the coils were built to withstand high temperature, high voltage operation. Design goals of 180°C and 10,000 v were set, and met.

The completed degaussing system performed as expected; but not in time for use with a flight spacecraft. Spacecraft degaussing was left as a post launch development study. The coils were used though, in degaussing the flight solar panels. In this application, the capacitor banks and surge suppression devices previously mentioned were not connected. Rather, the coils were series tuned to 60 cycles and an

adjustable tran former used as a power source. The coils were operated in this mode .r the post lau ch study on the magnetic stability of solar panels. The spacecraft degaussing study will be performed later this year.

That concludes the formal talk. I have some photographs showing the coils.

Figure 4 shows the pair of coils set up with a wooden frame between them. The wooden rails were used to slide the solar panels in place. The circular cutout in the wood was built for centering the spacecraft — the spacecraft adapter ring. For the spacecraft the rails are taken out, and the spacecraft adapter ring is slid down the indexing wooden rails on the sides until it is in the circular cutout. That puts the bus roughly in the center of the coil system.

There are a few things I haven't mentioned. There was a thermocouple built in the center of each coil; just to have the capability of measuring the temperature. In addition there was 100 turns of 20 gauge copper wire wound on the outer portion of each coil, more in the nature of a pickup coil for sensing any transients that might occur during failure mode. Each of the parallel legs is brought out to terminals at the top and was labeled by the manufacturer, and then each leg is tied to a surge suppression network. The high voltage signs were on there because they were used for the solar panel degaussing. Of course, for that application, we series-tuned the coils, so there were rather large voltages across the coils. Figure 5 shows the selenium surge suppressors and the capacitor banks.

They are rather large square devices, built by Westinghouse, called "volt traps."

Figure 6 shows the apparatus used to pick up the coils. You recall, for degaussing along the z-axis, we had to lay the coil down on its side, and then slip it over — actually, we put it down first, and then put the spacecraft inside the coil system. This is the apparatus used to pick it up and it has this large "wagon wheel" spoke system to keep it rigid.

They will take out the spokes, take out the frame, and the spacecraft will then be lowered down inside. This is all designed by Bob Norman. I am really not sure what the spacecraft will sit on. I don't know that, but it will be down inside there; more or less in the plane of the coil for this operation.

Figure 7 is a side view of the arrangement again. The large capacitor on the floor is for 60-cycle operation. We built a surge suppression device; it involved a diode bridge arrangement in parallel with the coils, and across the two load corners of this diode bridge was this large capacitor — polarized capacitor — it was always charged with the same polarity and the same voltage, at least the same polarity.

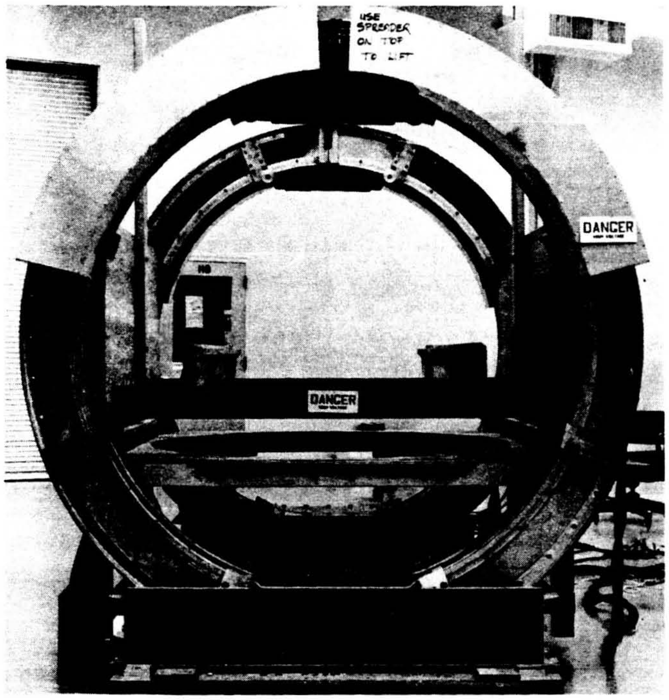

Fig. 4. Mariner Mars demagnetizing coils

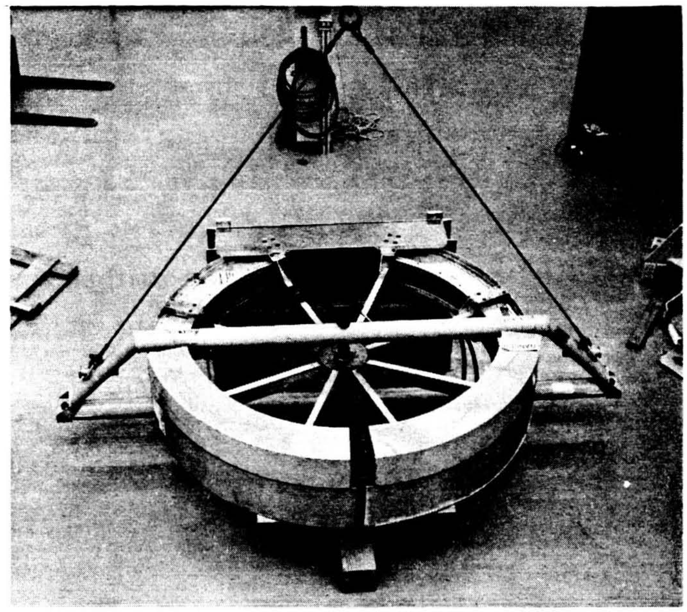

Fig. 6. Single Mariner Mars demagnetizing coil

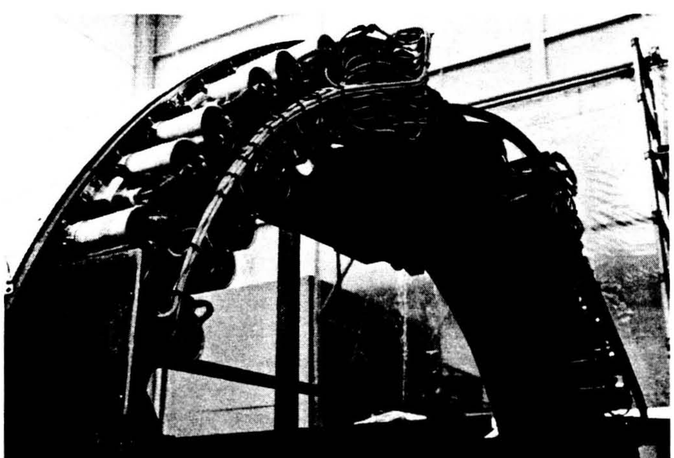

Fig. 5. Protective circuitry for Mariner Mars demagnetizing coil

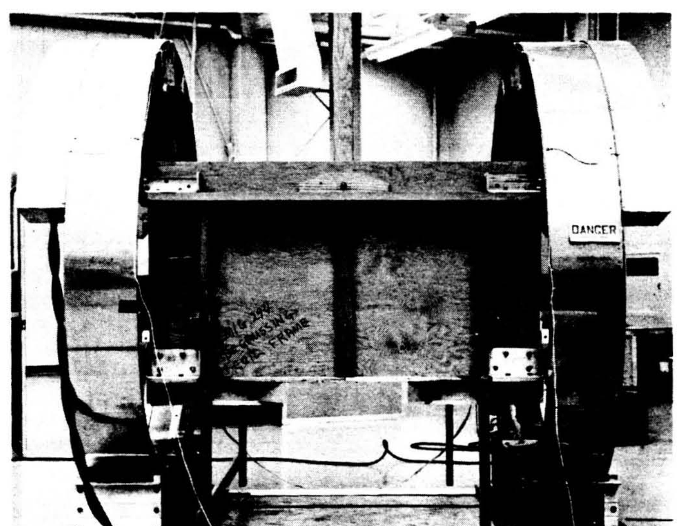

Fig. 7. Sideview of Mariner Mars demagnetizing coils

It wouldn't draw any current under normal operations, but for a large-voltage surge, it would shunt that current through the capacitor.

This coil form, I should add, is slit. It is not a continuous coil form. It is made of aluminum, and there was considerable machining to be done. I believe it was built here at JPL.

The overall cost of the coils, the winding operation, the wire, the electronics, the power supplies, and the programmers was on the order of $35, 000.

The wire that we used on the coils was number 9 square copper wire with a heavy polythermaleze insulation having a glass coating over that and class 200°C insulation. Between the layers of the windings there were about three layers of mica glass insulation. They were coated with a Durel varnish, class H varnish. If one of the 12 pair of legs did not fill up a complete channel width, then a spacer material was used so that the new winding was started on the next layer. I should add that there was a serious problem that Bob Norman had with the decision we made to go ahead and make the coils for high voltage, high temperature operations. 180°C operation required that the coils be baked several times. The worst one was 10 hr at 360°F. This degraded the strength of the aluminum coil form by about three-quarters, so this required that we take this in account for design of the coil forms, the thickness, etc.

QUESTIONS AND COMMENTS:

MR. PARSONS: If I understood you correctly, the presence of Earth's ambient field is being neglected entirely?

MR. FRANDSEN: Yes. We point the coils east-west; that is the best we can do. It seems to degauss the solar panels pretty well. The colored pictures you saw here were taken of the coils pointing east-west. The solar panel degaussing program was reasonably effective. It knocked the fields down to almost zero.

MR. PARSONS: Well, it should certainly knock something down. The question of how much it is going to knock it down will depend on how high it was to begin with. I will almost guarantee you will never get a perfect deperm treatment as long as you are sitting in the Earth's field.

MR. FRANDSEN: This is another possible application for this 100 turn, 20-gauge winding on there. Maybe we can buck down a little more with that. We really don't have a good capability of deperming in zero field, and I quite agree with you.

In trying to run some tests on some of the JPL equipment that is going to fly on the OGO program, I got myself involved in the perming and deperming business to try to meet the specifications, and I found if we depermed in the Earth's ambient field we were embarrassed because the depermed item was hotter than it was initially. So we finally depermed in the zero field, and it came out quite well. But we haven't run spacecraft deperming yet and we will see what problems arise there.

The solar panels were roughly a 25-gamma contribution each, they all added in the same direction and gave a total of 100 gamma at the flight probe. After deperming, it was down more on the order of a couple of gamma. So it was pretty effective.

CHAIRMAN GAUGLER: But they had a high aspect ratio, they were very long and narrow, so it was probably a natural for this type of operation.

MR. FRANDSEN: Right, and that could very well be the deciding factor in how effective it was. Yes, the solar panels were magnetized along the long dimension that, I think, is on the order of 7-ft, (little bus bars that long). So that was the source.

MR. PARSONS: That helps a great deal if you have a long axis.

CHAIRMAN GAUGLER: The spacecraft is going to be another problem.

MR. PARSONS: I gather that you said your spacecraft could not be tipped over, tumbled, or rolled physically.

MR. FRANDSEN: Well, I presented it as a handling constraint, that it wouldn't be degaussed or depermed with the spacecraft lying on its side. Actually, I suppose if we had had enough time to solve this problem, we could have; but it would have required special handling fixtures and we were so pressed for time to do this job, we had to limit it to what we had available in handling fixtures. So we decided to use the existing handling fixture, which was just a ring around the base of the octagon, and use slings to lower it into the coils for all three depermings.

MR. PARSONS: One reason I mentioned this is, if you are able to roll and tumble it, it gives you an advantage in deperming in Earth's ambient field. It gives you the one fighting chance to do a deperming in Earth's ambient and that is to tumble and roll about at least one, preferably two, axes while the treatment is going on. We attempted this with the OGO A, and we are planning on setting up to do it on OGO B.

We will have a deperm coil remarkably similar to yours in dimensions, 9-ft diameter Helmholz coil pair, but our fixturing is such that we can roll the spacecraft in, and tumble it and roll it on either of two axes. And, in addition, we will be in a zero field facility, on top of all that.

MR. FRANDSEN: This may be a serious problem that we just haven't encountered yet. We do have a few coils that we could probably adapt, for putting around this system, to bring the ambient field down to almost zero, but we thought we'd just go ahead and try it as it stands now and see how successful it is. You're right, it might be a real problem.

MR. PARSONS: One other comment on the subject of power supplies. One hundred amp DC has been used for many years for deperming of ordnance at NOL. The system being an amplidyne generator, rotating-machinery type thing, where you control the current on the field windings of the armature and lead out the armature current that is fed into coils. Such coils are already in use over there, one of which is perhaps suitable for a spacecraft deperm treatment. We may use it as a backup, in case our other system fails.

All I am saying is that rotating machinery-type control of big DC currents is old stuff.

MR. FRANDSEN: This is true. We were trying to use existing power supplies, but we couldn't find any existing power supplies that would meet the requirement for going down to zero. Getting close to zero is a problem, and we did not have an amplidyne, a motor-driven generator, available on the lab. We were fortunate enough to be able to buy off-the-shelf two programmable power supplies with very excellent regulation, which were controllable from full power down to zero. They go so far as to put an extra circuit in there, so that you can go through zero. It just seemed to have a lot of good features and was programmable.

MR. PARSONS: We are looking into the same thing. We don't like to leave any stone unturned. We will try them all sooner or later.

On the matter of getting down with a smooth approach to an absolute zero, we found two ways to do it. One way is with an amplidyne generator. There is always permanent perm in the pole pieces. So you shut off the thing and let it coast to a stop, and as it coasts down you get a nice, smooth approach to zero.

The other approach is one that I have been expecting the boys from NOL magnetic materials group to mention, and that is a removable primary or secondary

transformer, where you take the primary winding and physically pull it out of the secondary winding and carry it slowly away and turn it at right angles. This way you get an infinitely smooth taper down to zero.

MR. FRANDSEN: I should point out that I didn't give the full story on these programmers. I was just trying to give a brief explanation.

Actually, the motor-driven potentiometer, which is exponentially decaying as a function of time (the last portion is linear); we decided not to go on to infinity there with time.

MR. IUFER: Could you give us some background on why you selected logarithmic decrements?

MR. FRANDSEN: It's traditional. No good reason. You always see it in the textbooks that way, and it was easy to do. It is a constant percentage decrease each time.

The other method that I mentioned was the digital method where we select fixed resistors, this is a linear decay rate, it is not logarithmic, so we have the two methods.

MR. IUFER: What I am really trying to do is find out some other experience on this. As far as we are able to determine, a uniform decrement of 2 gauss produced about as good results as if you go to a much finer rate. This may be because we are demagnetizing samples that have relatively small length to diameter ratios. Therefore, we are not producing fields that carry them out to saturation or perhaps in some cases not even beyond the knee of the curve. So, therefore, our starting fields are not too high and a 2-gauss decrement seems to be adequate.

MR. FRANDSEN: We had no experience, at least in this regard, so we built the two capabilities.

MR. IUFER: I had another comment of the level on the last shot. I understood you to say that the last shot amounted to something like 0.2 gauss; is that correct?

MR. FRANDSEN: No, we are capable of going down — the requirement was 80-gauss peak to less than 0.1 gauss. We used 100 gauss as a design goal. We are still capable of going less than 0.1 gauss.

MR. IUFER: The question is this: since Earth's field has the value of 0.5 gauss, what advantage is there in deperming below this 0.5 gauss exposure that you will get as soon as you turn off the equipment?

MR. FRANDSEN: I don't know if there is any particular advantage. This was just a set of criteria we were working to. You could argue, though, as Mr. Parsons pointed out, there is an advantage to degauss in zero field. Even though you have picked the item out of zero field after it is through and carried it out into Earth's field, the fact remains that you get a lower remanence if you degauss in zero field. So, we were assuming that we could degauss in the east-west direction down to 0.1 gauss.

MR. IUFER: Ames' experience bears this out. We agree fully with you and Mr. Parsons, that the secret of success in deperming is not particularly the design of the uniformity of the deperming field, but in the design of the Earth's field compensating coil, so that you provide good compensation of Earth's field during the deperming operation. In fact, we have had very good success on short-run operations to place a large specimen in a low field coil and move across it with a very small deperming coil — as you would sweep across it with a whisk broom, wiping out the perm as you went, at 60 cycles.

In one particular experiment with 3 or 4 gamma at 3 ft, it was due to remanences inside on some of the cord wood modules that could not be erased with our 30-gauss capability of our 4-ft Helmholtz. So, we walked in there with a hand-held growler with 60 cycles and completely wiped it out so that we could not see it at 3 ft with a 0.1-gamma sensitivity.

So, I feel that the secret of success is wipe out Earth's field. The rule of thumb on this would be to reduce it by 2 orders of magnitude.

MR. FRANDSEN: I would like to add — I suppose that our most common way of deperming is to use a small tape demagnetizer and just have a switch on it, which is held in the on position, and take the package in hand and tumble it, as Mr. Parsons mentioned, as you go away. This seems to do a pretty good job of deperming.

SOLAR PANEL DEPERMING

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INTRODUCTION

The idea of deperming a magnetic material is certainly not new. What is new, is the idea of reducing residual perm fields to the gamma level. In the case of the Mariner Mars project, time did not permit a redesign of several assemblies that were producing excessive magnetic fields at the magnetometer experiment sensor. In the majority of these cases, substitute nonmagnetic materials lacked certain characteristics that would permit the immediate replacement of the offending material and there was insufficient time to find adequate substitutes or redesign assemblies to accommodate the nonmagnetic materials. Particular items where magnetic material had thus far defied substitution were in the welded nickel ribbon on circuit modules, springs in pressure regulators, stainless steel shafts and bearings, kovar on the solar panels, and invar in temperature sensitive bimetal spirals. There are, of course, components such as relays, solenoid cores, gyros, and motors that depended on magnetic material for their proper performance.

NATURE OF SOLAR PANEL PROBLEM

The assembly that contributed the largest field at the magnetometer was the solar panel. The offending elements of the solar panels are the kovar strips and wires forming the bus circuitry connecting the more than 7000 solar cells in a series parallel arrangement. Figure 1 shows these elements and how they are used on the panel. This material being a nickel-cobalt-iron alloy is ferromagnetic. Because of the way it is mounted on the solar panel, principally parallel to the long axis of the panel, it is particularly susceptible to perming when shaken in a magnetic field. Earlier, it had been found that the solar panels, as well as other assemblies containing magnetic material, were acquiring a perm when subjected to environmental testing on the electromechanical vibration exciters having a quasi-stationary field of from 2 to 20 gauss in the vicinity of the object being shaken; dependent on the particular shaker used and the direction of shake. This situation has resulted in a solar panel perm field magnitude at the distance of the Mariner magnetometer sensor of

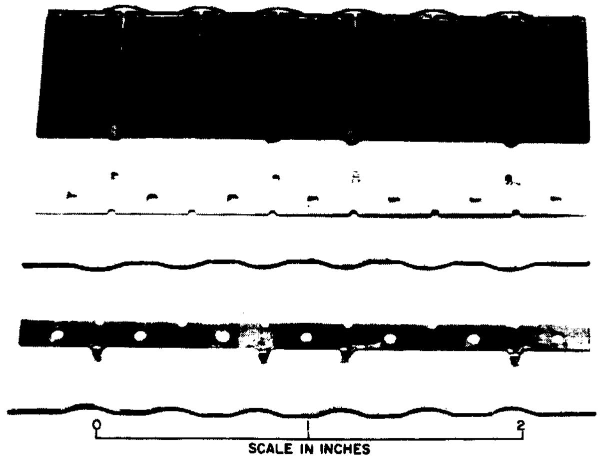


Fig. 1. Mariner Mars solar cells and Kovar buses

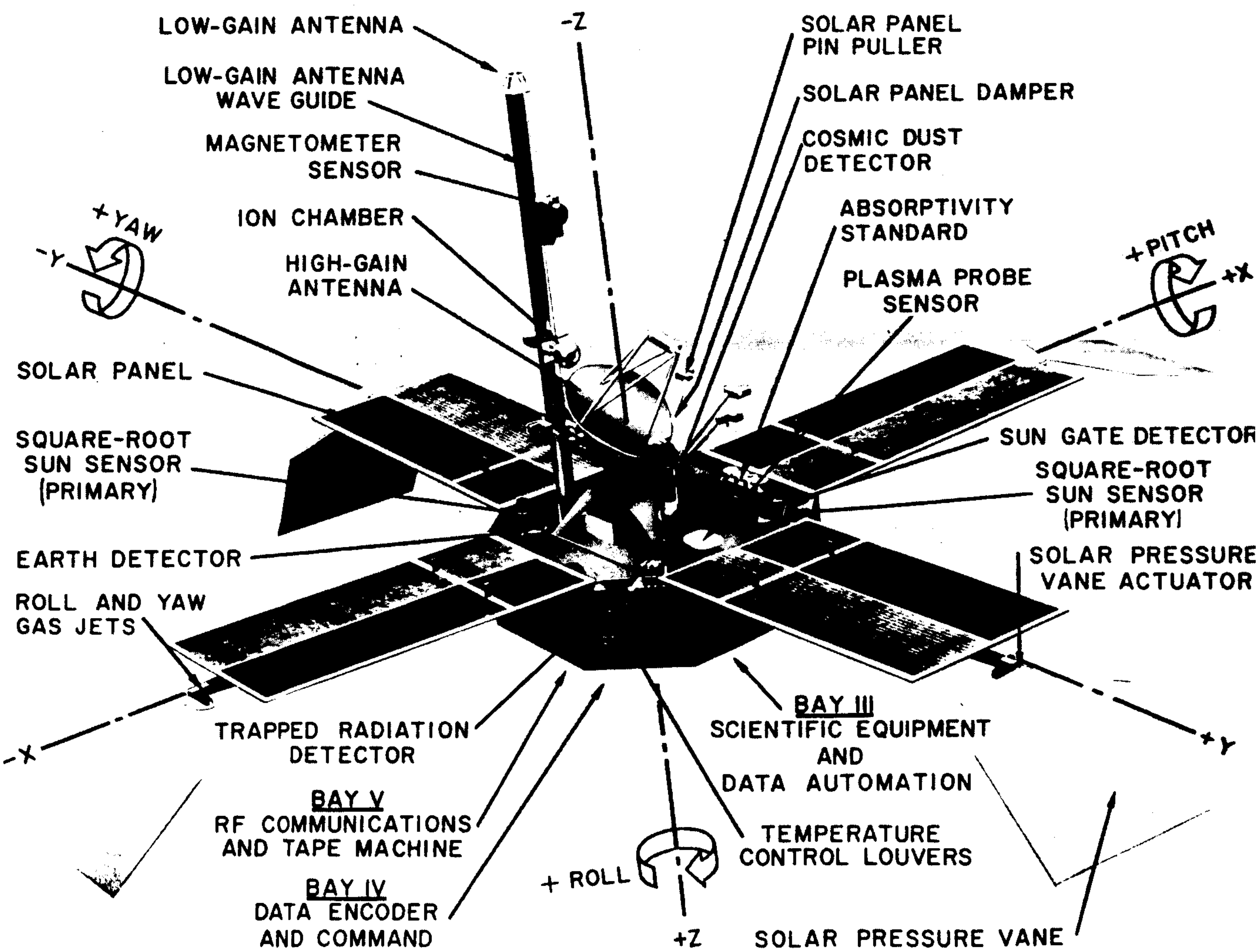


Fig. 2. Mariner Mars model showing component location and axes

from 2 to 30 gamma for each of the four solar panels carried by the spacecraft. The combined effect of four typical panels before deperming was determined to be $P_x \approx -16\gamma$, $P_y \approx -16\gamma$, $P_z \approx -95\gamma$.

As a result of suggestions for considering the deperming of the Mariner spacecraft, steps were taken to obtain a suitable degaussing or deperming facility as has been described in an earlier paper. Although the degaussing facility was designed for a frequency of 1/20 cps, to keep from inducing damaging voltages in the spacecraft circuits, time did not permit development of the necessary power supply or procedures. With considerable experience in deperming at 60 cps it was decided to adapt this degaussing facility to 60 cps operation in a series resonant circuit fed from a 220 v variac supply. Under 60 cps operation, the coil system was capable of producing an rms field on the axis, midway between the coils of about 23 gauss, increasing to about 30 gauss in the plane of each coil. Through habit, and custom, I have referred to the field in gauss rather than the more correct oersted and will continue this practice in the material to follow. With the solar panels contributing approximately three times as large a field (at the magnetometer) as the remainder of the spacecraft, and because preliminary tests with sections of a solar panel had indicated that the depermed condition was the more stable one, it was decided to direct all efforts towards a satisfactory deperming of just the solar panels.

SOLAR PANEL MAPPING

Before proceeding with a description of the deperming operations, and to clarify the discussions to follow, a brief description of the procedure used in mapping the solar panels will be given. This procedure is based on the technique described earlier for mapping the spacecraft in the Earth's field. The relative position of the magnetometer and the solar panels as well as the coordinate axis of the spacecraft, to be referred to in this discussion are shown in Fig. 2.

The panel is placed in a special handling frame so that the solar panel can be wheeled to the proper position with the magnetometer. It is first placed at the same relative position with the test magnetometer as the panel is to the science magnetometer on the spacecraft. For purposes of comparison, all measurements are for a solar panel in the -Y position on the spacecraft, which with the -X panel are slightly closer to the magnetometer than the other two panels. The first measurement is then followed by measurements at three other positions representing 180 deg

rotations about each of the three coordinate axis through the magnetometer position. The frame itself is capable of rotating the panel about one axis parallel to the long direction of the panel thereby supporting the panel at two different heights above the floor corresponding to the offset of the magnetometer from the centerline of the panel. With this arrangement, the panel is positioned so that the magnetometer will measure the components of the fields produced by the panel perm field and those induced in the panel by each component of the Earth's field. To simplify the measurements and provide a ready indication of outside disturbing influences, the Earth's field is initially nulled out in the absence of the panel.

By placing the panel in these four positions, pairs of oppositely induced fields will be produced in the panel so that, with the proper algebraic manipulations of the component field measurements, the induced fields will cancel out giving a measure of the perm field in a field free environment. In addition, errors are reduced because the measurement is essentially a four measurement average. The resulting perm field components then are the components of the field at the spacecraft magnetometer sensor in spacecraft coordinates. The measurements are accurate to less than 1 gamma if outside disturbances are held to a minimum. This is not too difficult to achieve with reasonable precautions. Quite frequently, mapping has been done in the evening hours to avoid interference with other work in the area. The actual repeatability of the measurements is a function of the care in positioning the panel in the mapping dolly and in positioning the dolly on the marks. It is believed that this has resulted in variations in the measurements of from 1/2 gamma to about 5% of the reading above 25 gamma. At the position of the spacecraft magnetometer, the field, because of the panel, is principally in the -Z direction with the result that the -Z component is very nearly the same as the total field and, in the discussion to follow, may be the only component to which reference is made.

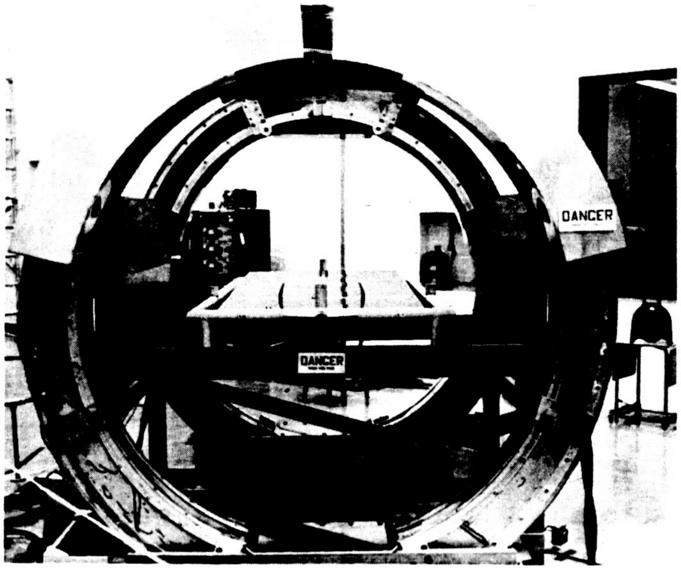
PASADENA DEPERMING OPERATIONS

Returning now to the solar panel deperming itself, there was some concern that either the deperming operation or the solar panel handling incident to it would irreparably damage the flight solar panels. Consequently, extra precautions were taken to ensure that the operation would be entirely safe and have no adverse affect on the spacecraft. Failure mode tests were imposed on the deperming system to ensure that transients would not damage the panels. These tests consisted

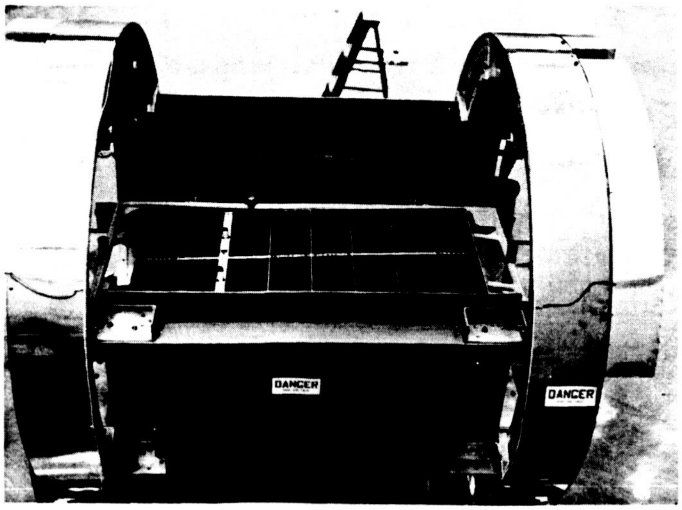
essentially of a series of open and short circuit conditions in the series circuit and a loss of power at the variac input, all at maximum applied voltage. This latter test did cause a minor problem because of a high voltage transient across the variac secondary, damaging a voltmeter; however, no transients could be observed in the magnetic field from this cause. Following these tests and a determination of the magnetic field versus the input current, deperming tests were made on Mariner type approval solar panels to develop a suitable technique for handling and deperming the panels.

Because the type approval panels were initially found to have relatively low perm fields, on the order of 5 or 6 gamma, it was decided to attempt perming them to a higher level using the 60 cps degaussing field in the presence of a biasing field from two permanent magnets. Using this technique, it was possible to induce perm fields of 16 gamma in the panels. This fact necessitates having the coils oriented with their axis normal to the Earth's magnetic field during the deperming operations. In deperming the panels, the panel was inserted into the coils on a special frame, the field was increased to a maximum value and held at this value for 10 min before it was gradually reduced to zero and the panel removed from the coils. This arrangement is shown in Fig. 3. Schedule limitations did not permit more than a cursory determination of the effect of time or maximum field level in this operation. In the few cases where the time was considerably less than the 10 min, complete deperming was not obtained although these attempts were also accompanied by a lower field level. To reach a satisfactory solution more rapidly, the deperming time was kept at 10 min and only different levels of deperming field were tested. To preclude any possibility that the panels could be permed beyond our capability of deperming them, we restricted our deperming of solar panels to use of not more than half of the maximum capability of the coils.

The first panel to be depermed was done at 11.5 gauss. Initially, the field at the magnetometer was mapped at $P_x = P_y = 0\gamma$, $P_z = +10\gamma$. After the deperming, the field was mapped at $P_x = P_y = 0\gamma$, $P_z = -1\gamma$. This panel was then subjected to a 21 gauss deperming field for 22 min. The resultant field was -0.6γ. The panel was then permed to $+16.5\gamma$ and redepermed at 2.5 gauss. The resultant field was $+0.6\gamma$. Again the panel was subjected to the maximum field for 30 min, with the resulting field -1.1γ. The panels were then thoroughly checked in electrical-sunlight tests to determine if their performance had been degraded. No



a. end view



b. top view

Fig. 3. Demagnetizing Mariner Mars solar panel

apparent adverse effects could be detected in the optical or electrical quality of the panel.

CAPE KENNEDY DEPERMING OPERATIONS

The degaussing facility was shipped to Cape Kennedy so the flight solar panels could also be depermed. At the Cape, the failure mode tests were repeated to ensure that operation was satisfactory and that there had been no change because of the shipment. A final type approval panel was then depermed to test the procedure to be used with the flight panels. Because of the earlier success at 2.5 gauss, this amount of field was first tried for only 1 min. This deperming reduced the Z component from -19γ down to -10γ. The panel was again depermed at 5 gauss for 10 min further reducing the field to -2.5γ. Finally, this panel was redepermed at 10 gauss for 10 min, reducing the perm field to -0.5γ. With no time being available to further investigate the best deperming level or the effect of time, it was decided to carry out the deperming of the flight panels at this 10 gauss level for 10 min to ensure a satisfactory deperm. In deperming the flight panels, after the panel was placed in the coils, the AC field was increased to 10 gauss in about 10 sec, held at 10 gauss for 10 min and then slowly decreased to zero over about 40 sec. The panel was then removed from the coil system. Two spare flight panels and the eight Mariner III and Mariner IV panels were depermed in this manner. Typical results are shown in Table 1.

In spite of these gratifying results, there still remained apprehension about the stability of the depermed panels both in the Earth's field and in the essentially zero field found in interplanetary space. It has been of some reassurance that several depermed panels have been shipped between Pasadena and Cape Kennedy with less than 1 gamma change in the perm fields over several months. Table 1 shows a change of about 2 gamma for permed panels under these conditions.

POST LAUNCH STUDIES

To learn something about the magnetic stability of solar panels, a modest study has been conducted over the past 2 mo. Of particular concern have been answers to the following questions:

Table 1. Typical results of solar panel mapping

Panel	Spacecraft position	Before deperming						Depermed		
		May 7, 1964, Pasadena			Oct 4, 1964, Cape Kennedy			Oct 10, 1964		
		P_x	P_y	P_z	P_x	P_y	P_z	P_x	P_y	P_z
006	+X	-1.25	-2.75	-21.5	-1.5	-2.5	-19	-0.25	0	-0.25
007	+Y	-3	-1.5	-20.75	-2.5	-1.25	-19.75	0	0	-0.25
008	-X	-8	-3.5	-26.5	-8	-3.5	-24.75	0	0	-0.5
009	-Y	+0.5	0	-2	0	-0.75	-3.75	0	0	-0.5

1. Does the magnetic field of the Mariner Mars spacecraft change incident to launch vibration in the Earth's field?
2. Does the field of a depermed solar panel change with time in the low field environment of interplanetary space?
3. Is there an appreciable change in field of a depermed panel incident to long time storage in the Earth's field environment while awaiting launch?
4. Is a permed panel more stable than a depermed panel?

To gain more sensitivity in the panel mapping, panels were mapped both in the regular -Y position (which is 57-1/2 in. from the magnetometer) and in a plane 18 in. from the magnetometer, both near the hinge end of the panel and opposite the approximate midpoint of the panel. These latter positions have no significance insofar as the Mariner Mars spacecraft is concerned, but permit observation of a change in perm that might not be observable at the greater distance.

The first part of this study involved an investigation of the effects of the environmental vibration testing on solar panel perm. Because it was known that the perming of the panels in the shaker field was occurring, it was considered possible that flight solar panels might acquire a perm during launch and powered flight because of vibration in the Earth's field. One of the panels was subjected to different levels of field to determine the relation between the acquired perm and the perming field. To perm a panel, it was placed in the coils and subjected to 2 or 3 cycles of 1/20 cps operation at the desired level. These results are shown in Fig. 4 where the total field is within about 10 deg of the z direction. The panel approached saturation at about 20 gauss corresponding to a resultant perm field of about 165 gamma. In individual vibration tests in the three coordinate directions, it was found that the highest perm occurred in the shake parallel to the long axis of the panel that was also in the direction of the shaker field. The perm field because of vibration in the long direction falls between the 50 and 60 gamma points, which is consistent with the measured field of the shaker. No direct comparisons can actually be made because the shaker field is highly divergent running from about 10 gauss at the center of the lower hinge edge of the panel to about 4 gauss at the center of the panel with a reversal of field near the lower corners of the panel. From this study it was also observed that a 1 gauss field gave a perm of about 7 gamma, while 0.2 gauss produced about 2 gamma. The results of these perming operations were quite repeatable and reasonably predictable. At both the 5 and 10 gauss levels, panels were

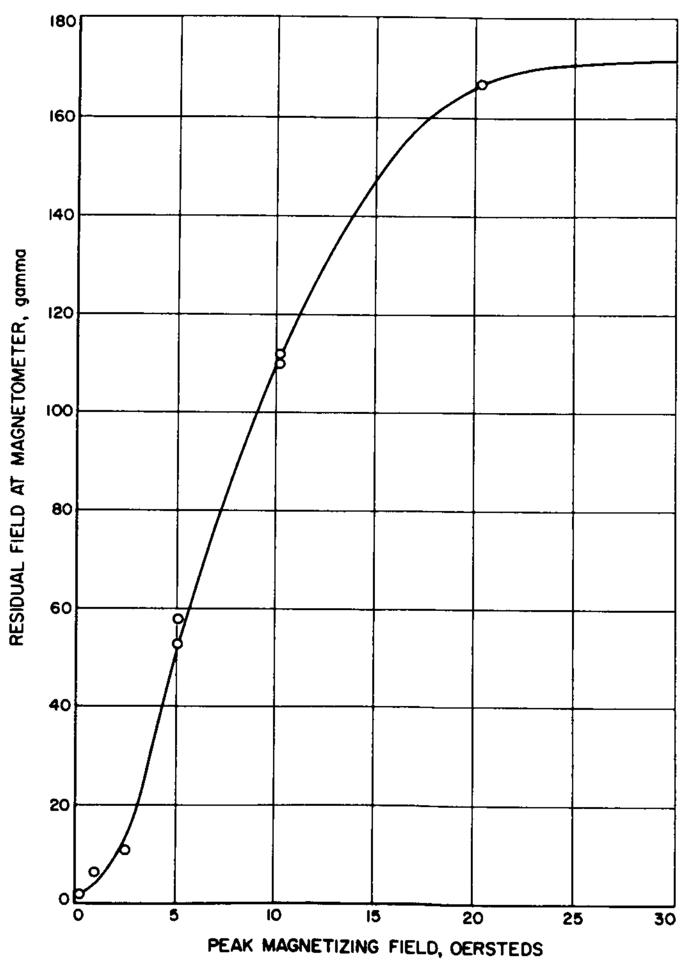


Fig. 4. Magnitude of residual field of permed solar panel at science magnetometer for panel in -Y position on the spacecraft

permed twice and the panels mapped. Where the same panel was permed the second time, the perm fields were within a few percent of each other. When two different panels were permed to the same level, the difference was as much as 10% in one coordinate. From these results, it appears that the effect of vibration of the panels in the Earth's field could cause a change in the solar panel perm field as much as 5 or 6 gamma in the z coordinate direction if the field were aligned with the panel. With the normal spacecraft orientation on launch and the direction of flight, the perm produced by the Earth's field should be very slight, 2 to 3 gamma. A big unknown in this discussion is the field of the gantry and umbilical and their affect on the solar panel fields during launch or work on the spacecraft on the pad.

The panel was also shaken over different frequency band segments of approximately 1 octave to find a correlation between the frequency of shake and the amount of perm. In normal vibration tests, the frequency range covers about 7 octaves beginning at 40 cps. The perm was almost identical for each of these bands. Vibration tests on permed panels only highlighted that the resultant perm in a panel is a function of previous history. All the permed panels had the perm field markedly changed by the subsequent vibration with its attendent field; however, there was no correlation between the final perm field and either the initial perm or the perm acquired by a depermed panel solely because of vibration.

The results of the tests on the panels that were stored have been somewhat inconclusive. Mappings for -Y position have shown essentially no change in the depermed panels, either in a zero field or in the Earth's field. The panel that was permed to initially have a Z component of perm field of 58 gamma, decreased by about 35% in the first 7 days of storage in a shield room zero field environment. At the closer mapping points, the change was less than 10% for the total field. The next 6 days the field increased slightly but showed a decrease the following 2 wk. The best indication of the affect of storage and a moderate amount of handling of the panels is still the comparison of measurements made at the Cape and at Pasadena.

Incident to this study, the panels have been depermed more than 10 times at 60 cps following the same procedure used with the flight panels and with similar results. A considerable amount of experience has also been gained in perming and deperming the panel at 1/20 cps. At 1/20 cps the panels have been depermed from the saturation level with as much as 80 gauss. Deperming from this level was done in about 35 min. Thus far, time has not permitted a thorough investigation of the rate of decrease or the best decrement of the decreasing deperming field. In the

deperming operations at 1/20 cps, the perm field was reduced to about 1.5 to 2 gamma each time. It was found that the power supplies could not be brought to a sufficiently low output by the programmer and that when the programmer was at zero there still remained about a 0.2 gauss field that caused this level of perm. This problem is now under investigation with the power supply manufacturer.

GENERAL REMARKS AND CONCLUSIONS

These studies have served to point out that the stability of each assembly must be individually investigated. In effect, we are investigating a composite of the magnetizations of the magnetic materials in an assembly and its stability. Here we have been studying the magnetic behavior of a single material, kovar, and its stability under certain conditions designed to simulate the spacecraft environment. A different material on the solar panels or a consideration of the perm field of other assemblies on the spacecraft would be expected to behave in an entirely different manner. Our studies have produced the following conclusions:

1. Deperming of solar panels is very effective. The perm field can be reduced to be negligible at the magnetometer sensor.
2. The depermed panels appear to be quite stable in the Earth's field and in a near zero field. Even when permed in a field of the order of magnitude of the Earth's field, the resultant perm field at the magnetometer is only a few gamma.
3. A panel permed to have a field at the magnetometer of almost 60 gamma, showed a definite loss of perm in 1 mo when stored in a near zero field when mapped weekly.
4. Vibration or shaking of magnetic materials on electromechanical shakers with their resultant fields requires later deperming, which should be at the latest possible time before launch.

From these conclusions, the deperming of other larger items including the entire spacecraft appears quite promising.

It remains to more thoroughly investigate the magnetic fields associated with the launch environment to determine the effect of launch vehicle, gantry, and umbilical tower on the spacecraft perm fields. Even with the uncertainty about perm stability, the overall situation has been improved by the reduction in total field at the magnetometer and the stability seems to be better in the depermed condition. The

only real solution to the solar panel problem, as well as the basic problem for the entire spacecraft, is to continue the search for suitable nonmagnetic replacement materials for the various applications. So long as our spacecraft contains magnetic material and it is exposed to magnetic fields such as those encountered with the vibration exciters, deperming will be a necessity if we are to approach the condition of having a magnetically clean spacecraft. We are replacing our shakers to obtain those with lower magnetic fields although there is still room for improvement here. We also resort to deperming of our tools and the use of nonmagnetic tools to keep from inducing fields in the magnetic materials we must carry on the spacecraft.

In the meantime, we will undoutedly continue to find it necessary and desirable to deperm various spacecraft and their assemblies. Towards this end, each problem should be subjected to a thorough investigation of the expected stability and the degree of reduction in the field that is possible by this technique. We now have a program planned to investigate the effectiveness of deperming and the perm stability of the remainder of the Mariner Mars spacecraft; that is, the bus structure without the solar panels. This deperming will be carried out at 1/20 cps and with the deperming field successively in each of the three coordinate directions. This study is presently planned in the next few months on the proof test model of the Mariner Mars spacecraft. Here we are faced with more than just one magnetic material as well as several unshielded hard magnets. The spacecraft field of about 30 gamma in the -Z direction seems primarily to be due to the welded nickel modules and temperature control louvers. With deperming only a partial solution to our problem, the ultimate solution still appears to be the reduction and substitution of magnetic materials.

OPEN DISCUSSION

MR. PARSONS: One question. I wanted to be sure I understood in which building it was that you were doing the mapping, and what was the background noise level that you had to contend with during the mapping?

MR. BASTOW: The variation is about 1 to 2 gamma. This is recorded on a Sanborn recorder. So, even with an AC field on there, you are able, so long as the drift is not great, of picking out within 1-gamma accuracy.

MR. PARSONS: In the magnitude of the depermed field with a 60-cycle source applied, is the magnitude you stated an rms magnitude or peak magnitude?

MR. BASTOW: This 23-gauss capability of these coils is rms.

MR. PARSONS: That's what I expected. So the actual shot is probably what?

MR. BASTOW: We depermed at 10 gauss, which would then be a 14-peak.

MR. PARSONS: The other point that disturbs me is the time interval. I have no data of my own, certainly, that would indicate that I would gain anything by holding the deperm treatment on for 10 min. I would say that at the end of 10 sec it has done everything that it is going to.

MR. BASTOW: Well, I would be inclined to agree, except that we didn't have time to investigate this. Every time that we did reduce the time, unfortunately, we also reduced the field but we did have trouble. We didn't really have time to go into it.

MR. PARSONS: That's the best I can figure.

MR. BASTOW: I also might add that, because these were so delicate, there was no harm in letting it sit for 10 min so it wouldn't be a hurried-up process. This wasn't a situation where we put the thing into the coils and then pulled it out. We put it in, turned on the field, turned off the field, and then withdrew the panel.

MR. PARSONS: There is a certain psychological factor there, I realize. People think you are crazy if you give them a 5-sec treatment and then tell them to take it away.

MR. IUFER: The thought just occurred to me, instead of carrying the deperming system to the Cape, how about using some of the Mumetal shield technology for transit cases, so that the cells would never be exposed to a perming field? Then you could rely on the deperm that occurred here before shipment.

MR. BASTOW: I am afraid that you would still be worried about someone who might come along with a magnitized screwdriver.

MR. FRANDSEN: In this case the panels were already at the Cape, the flight panels. We had to move the deperming system down there for that reason. I think I can add a comment to pacify Mr. Parsons here too.

During the final mapping at the Cape of the flight panels — each of the four test positions — we made a reading and then rolled the panel away to infinity, we checked our zero, rolled up to the next position, rolled away to infinity; so we had even better closure for this reason.

MR. KELLER: Keller, Goddard Flight Center. I was wondering, would you advocate deperming all spacecraft before the launch at the launch site? Is this a policy that you subscribe to?

MR. FRANDSEN: This is the last ditch thing you do. Of course, you try to keep it clean to begin with, but if you have to deperm....

MR. KELLER: Can you do that at the manufacturing site and ship a completed spacecraft without getting in trouble with contamination?

MR. FRANDSEN: I don't think we know for sure. We could probably use, as Mr. Iufer mentioned, some sort of shielding technique if you did this, for shipping purposes. We really haven't the experience to answer that for sure.

MR. BASTOW: I might add that we, of course, are interested in doing this as late as we possibly can. We don't know what the panels might be subjected to on the way across country, in the way of either fields or vibration, or both. So the later we can do the deperming, the better off we will be.

MR. NORRIS: The question came up about why hold the degaussing field on for any length of time, and I don't know the first thing about it, but there is a thing called anhysteretic demagnetization where you can demagnetize something that is actually permed to a higher level than your demagnetizing fields. In other words, you start and ring it down along its demagnetizing curve. I believe that has something to do with the reason they did hold the currents on for an extended length of time. Although I don't propound to be an authority on this; there might be someone here who is.

MR. IUFER: We don't have too much information on kovar, quite a bit has been done with steels that are used in ships. In this case, the induction field has been sampled during the application of a deperming sequence. The results, as I recall, are that once the eddy currents have completely died away, the field has

completely permeated the sample. Any additional wait after this has not shown any significant or measurable improvement in the operation; so, essentially, the eddy current characteristics define the response time of the specimen.

MR. BAKER: Baker from Sperry Phoenix Company. I believe you mentioned the use of shake tables that produced lower fields to avoid perming up the solar panels in testing. Are you looking at hydraulic shakers or some other type?

MR. BASTOW: No, these are the same type. I am not sure of the history behind this, but I understand that these shakers were obtained specifically because they did have lower fields. However, in conducting the solar panel studies, I measured as high as 10 gauss at the lower edge of the panel. It ran up to about 4 gauss at the center of the panel — this is with the panel vertically on top of the shaker, shaking in the vertical plane. It curved around and reentered down in the corners of the panel at about 2 gauss.

They have had shakers here that ran as high as 35 gauss and, unfortunately, the shakes that they give to the various components aren't always on the same shaker, or haven't been in the past; consequently, it is quite a bit of effort to trace down the specific history of the various assemblies insofar as their shaking is concerned.

Also, there doesn't seem to be any particular order in conducting the shake. I found that in the vertical direction it received a high perm and in the other two axes (where it is farther from the field), the perm wasn't as high. The resultant perm depended considerably on which shake came first.

MR. IUFER: I'd like to put out a suggestion to those who are concerned with perming and the efficiency of perming. It essentially involves the standardization of what is good and what is bad. We've said that gamma before and after deperming was good, but this is still by no means quantitative, so what I am submitting at this time is that (perhaps when deperming is being undertaken) to start out with a deperm, then expose a specimen to a standardized exposure of perhaps — we were suggesting 25 gauss. Record this number. Then, in terms of the magnetic induction you got from 25 gauss, then say what the induction was after deperming. Did you get 100% reduction? A 95% reduction? Then you can assess whether or not you are doing as well as perhaps some other center or some other agency or some other company.

We find that 90 percent deperming can be done quite conveniently. If you are not getting this much reduction, you can start looking at the meters, the switches, and the what not.

MR. ADAMSKI: Adamski from Aerospace. Is there any correlation between the g level on which you perform your vibration tests and the amount of perm that you put back in during the vibration tests?

MR. BASTOW: I didn't really go into that at all. In conducting these vibration tests at the different frequencies, there was a different g level used. Actually, what I did was take the standard flight acceptance test that we have here for the panels. I was trying to find out what the panels were doing, actually, under the normal treatment that they would receive. Then I took different octaves of this range and gave it whatever g level it had for that particular frequency band. So the g did go up with the frequency; it was somewhere around 5 g, at the high frequencies around 1500 cycles, and, if my memory serves me, it was around 2g at the 50- and 60-cycle range.

MR. ADAMSKI: Is there any possibility of a correlation between g level? You say you tested to the flight acceptance test level that is on the order of magnitude 3 db down from the actual flight level that you may see? In other words, the proof tests are much higher than the flight acceptance tests?

MR. BASTOW: That's correct. I don't know what the level is in relation to the expected flight environment, however. My conclusions are that I was above a certain level of vibration, and the field was what controlled the amount of perm rather than the vibration.

MR. IUFER: I can toss in a little experience. Our experience is that, in shaking (define shaking as purely mechanical shaking), the specimen will acquire a magnetization that corresponds to the ambient field level for that particular orientation. You have essentially two factors: one is time and one is vibrational level.

If either one of these is high enough (or long enough), then the specimen will reach an equilibrium magnetization beyond which it doesn't seem to be affected. I might also mention it appears that energy is required for the material to orient with this steady-state external field, and you can get very slow changes with time just because of thermal agitation in the material. This can be a source of energy for this orientation process.

What I am essentially saying is th beyond a certain point, increasing the amplitude or the g of shake does not furti increase the level of magnetization.

MRS. EBERHARD: Were any other particular measurements made of the illuminated panels to find out how much of the field was due to currents in the panels?

MR. CHRISTY: We did make measurements on current loops. The panel circuitry was laid out in an optimal array. The field contribution at the sensor because of a full load on the panel with one Earth's constant illumination gave no rise whatsoever at the sensor. No near field measurements were made.

MRS. EBERHARD: What was the load current?

MR. FRANDSEN: We performed these measurements at the mesa with a sun-lit panel, and there were contributions from individual sections of the panel. But the sections were wired so that the currents were canceling. A fully-illuminated panel with all sections operating gave a very small contribution that was (the number escapes me — but I think it was) on the order of maybe, say, 1 gamma/amp of total panel current.

If, for some reason, we were to lose one section of the panel, say one-fourth of the panel, because of the impact of a micro meteorite, then we would have a jump. We know what the jump is, because we have looked at the panel (individually), section by section. There is some small contribution, yes.

MR. PEIZER: I am not sure — I assume that this has been considered, but I just want to check whether the eddy currents induced by the motion of conducting materials have been measured or considered. For example, that aluminum there.

I think this was mentioned, but I just wanted to confirm it.

MR. FRANDSEN: It was, I suppose, briefly considered. We really pretty well ignored it, I can say, because there really aren't any moving devices on the spacecraft (other than the scan platform) during the so-called current loop tests on an operating spacecraft, we monitored the flight magnetometer that was properly nulled down to a low field value. We monitored the magnetometer while various substances were operating, including the scan platform motion. We have a record of these effects, these interactions, when you turn on the radio subsystem or the battery or you operate the scan platform, or the TV. We had a record of what these contribute to the flight magnetometer reading.

But, by and large, the spacecraft itself is attitude stabilized — it is not a spinning spacecraft, by any means.

CHAIRMAN GAUGLER: It is in a 5-gamma environment in interplanetary space.

MR. PEIZER: Just a small comment on that. Unless the thing is calculated, it had better be checked. We ran into this with a sweeper, and the first reaction was, that won't have any effect. It turned out to be something horrible.

CHAIRMAN GAUGLER: There are some mysterious gas consumption problems and glitches in the attitude control systems that people are looking into, esoteric things like that — and try to explain it.

MR. PEIZER: I did not mean anything esoteric. This is straight induction of electric current in conduction materials, and just looking at it you might say it is not moving very fast or not very much material, but somebody better figure it or check it.

MR. CHRISTY: I don't mean to contradict what Mr. Frandsen said, but I believe that some of John Casani's people have made calculations along this line.

MR. PARSONS: There have been a couple of theoretical studies made of the spinning spherical shell in ambient fields. We, at Goddard, hope very soon to have the rotating vector capability that, we hope, will permit us to study some of these things. It is difficult, if you have to spin mechanically, to get good readout. We are going to attempt to look into some of this, and I know exactly the problem that Mr. Peizer is referring to. It can get away from you.

We did do some studies on flywheels spinning in Earth's field, back in the old days, and found that you can get quite a sizable field if you spin up to high speeds. Of course, these things are mostly 30 rpm-type stabilization spin rates.

MR. CONNOR: Ben Connor from JPL. The only experience that I have had with this was on mapping the Mariner-Venus spacecraft. We had a jig, for holding the spacecraft in a horizontal position, that was made of fairly massive aluminum I-beams. The members of the truss were not insulated from each other, so there were possibilities of large current loops. We found that we couldn't make any accurate measurements under continuous rotation of this gear. The fields because

of induced current were so large that we had to stop every 15 deg and make an incremental-type measurement. Even with very slow rotation, the induced field is sometimes two or three times the perm we are trying to look at.

MR. NORRIS: I want to clinch a rumor before it gets started. The scan platform on Mariner Mars didn't show up any measurable field when it was moving in the Earth's field, which was quite a bit larger than the interplanetary field, and we did get another mapping on it during interplanetary operation, so everyone can forget about that as a source of problems on the measurements on Mariner Mars.

MAGNETIC SHIELDING OF SPACECRAFT HARDWARE

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The use of permanent magnets and strong solenoidal magnetic fields in latching relays, RF circulators and circulator switches, traveling-wave tubes, and various types of motors on magnetometer carrying spacecraft cannot be avoided. Pairing or preferential placement of magnetic source components frequently exceeds the physical design envelope in weight and/or volume. Under these circumstances the only corrective action within the present state of the art, and short of subassembly redesign, is to magnetically shield the source field.

Shielding for the purpose of containing or excluding magnetic fields is not a new art. A search of the literature yielded several successful shielding efforts, none of which could even remotely apply to a 500 lb spacecraft requiring total magnetic field stability of a few gamma. Mariner II experience had led to the expectation of a spacecraft magnetic field instability of at least 10% of the total spacecraft magnetic field. For many assemblies the percentage was higher. With the Mars launch opportunity almost upon us, subassembly redesign for most equipment was impossible. Mariner Mars spacecraft magnetic field stabilization through field reduction by magnetically shielding certain subassemblies was undertaken as a last resort effort.

Magnetic shields were designed and fabricated of Mumetal and moly-permalloy (4-79) to contain the relatively long duration pulsed or the permanent magnetic fields associated with electromagnetic devices on the Mariner Mars spacecraft. The design and fabrication philosophy practiced throughout these shielding experiments were:

1. As nearly as possible, shield only the magnetic source entity.
2. Make the magnetic shield path as complete as possible yet simple by surrounding the source on all sides, minimizing the number of openings, and by using flat or large radius surfaces, large radius corners, and by keeping the three coordinate dimensions nearly equal.

3. Where fabrication requires welding, use strips of the shield container material for welding filler.
4. Hydrogen anneal the shield at 2150°F after all fabrication and in accordance with standard annealing schedules for Mumetal and moly-permalloy.
5. Allow no bending, drilling, or reshaping of the shield after annealing.

This presentation will review the shielding efforts on three major magnetic interference problem areas encountered on the Mariner Mars spacecraft: latching relays, the motor driven power switch, and the RF circulator switch.

LATCHING RELAYS

The first of these areas, latching relays, was known from Mariner II experience to be unstable moderate magnetic field sources that contributed substantially to the magnetic field at the magnetometer sensor. With approximately 100 relays proposed for use in the Mariner Mars spacecraft as the stimulating omen, a study of magnetic fields of typical spacecraft relays and shielding methods was initiated in August 1963. Table 1 lists the field strengths measured for the relays indicated.

Table 1. Relays without magnetic shielding

No.	Relay	Field strength, gamma at 12 in.	
		Before vibration	After vibration
1	Latching	77.9	66.4
2	Latching	80.9	106
3	Latching	85.9	76.6
4	Nonlatching	0	~0.5
5	Latching	145	---
6	Latching	13.5	---

All measurements were made in the Low Field Facility on the JPL mesa. The JPL Low Field Facility and measurement techniques used there have been described in detail in various papers by A. A. Frandsen, B. V. Connor, and others. A single axis Fanselau coil system is used to create a nearly zero magnetic field environment. The test sample is rotated on a turntable in the center of the coil system. The turntable is coupled to one axis of an X-Y plotter. A single axis fluxgate magnetometer is used as the magnetic field pickup, which, in turn, is recorded on the second axis of the X-Y plotter. Thus, the test data is recorded as an analog planar magnetic mapping at some fixed distance from the test sample.

First efforts to reduce these fields resulted in the construction of five-sided rectangular shield cans of 0.050 in. thick Mumetal shown in Fig. 1. Each relay magnetic shield can weighed approximately 0.35 oz. The effectiveness of these shields and their stability under vibration at 14 g rms noise in an 18 gauss magnetic field can be seen in Table 2.

Table 2. Relays in Mumetal magnetic shields

No.	Relay	Field strength, gamma at 12 in.	
		Before vibration	After vibration
1	Latching	5.0	4.5
2	Latching	6.5	6.0
3	Latching	4.0	2.5
4	Nonlatching	~0	~0

Comparison of the data shows that a shielding factor 12 to 20 can be obtained.

Although these Mumetal relay shields were recommended for use on Mariner Mars spacecraft flight relays, the shielding gain produced a relatively large weight penalty that was unacceptable.

Again, in June 1964, interest in relay shielding was revived through the design of Advanced Mariner subsystems. Three cell shields as shown in Fig. 2 were fabricated. Though shielding factors were found to be comparable with the previous single cell shields, a magnetic shunting effect was experienced that inhibited the change of state of the relay. Electrical evaluation of the relays in the single cell

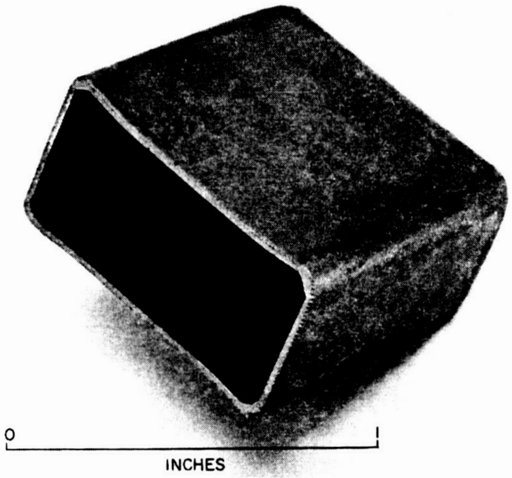


Fig. 1. Five-sided single cell relay shield can

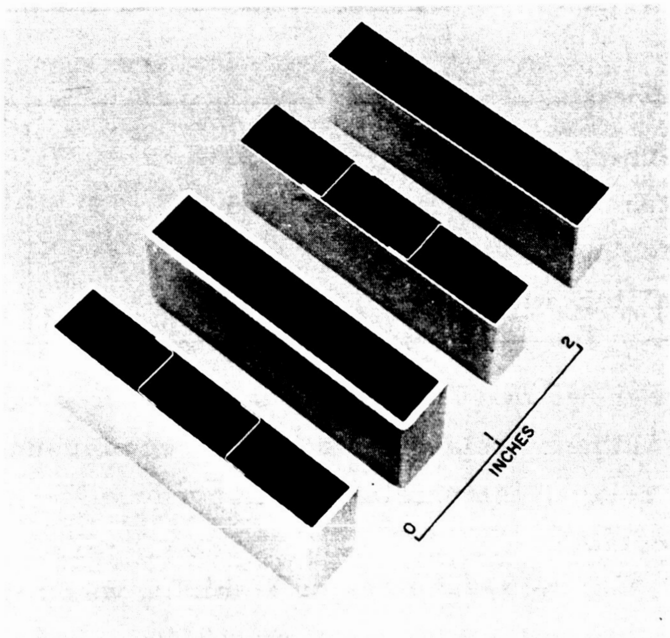


Fig. 2. Three cell relay shield cans

magnetic shield confirmed the shunting effect and resulted again in relay change of state inhibition. At this point the wiseness of the decision not to fly the shields on the Mariner Mars spacecraft was realized.

This problem generated an even greater interest in the magnetic circuitry of the relays and how the radiated magnetic field might be reduced. It also demonstrated the basic concept that a magnetic source should be shielded directly rather than shielding the entire black box.

Evaluation of the internal magnetic circuit typical of Mariner flight type relays yielded several marginal magnetic connections. The two obvious high reluctance paths occurred at the rocker element gaps. These two paths were considered first for flux leakage correction. Although a shield covering five sides of the relay produced excess shunting, perhaps shunting localized around these gaps in some best geometrical array would produce the desired field reduction. The gap-shunt or patch work shields indeed do produce some field reduction. Fourteen pairs of these shields were fabricated in two thicknesses and various geometric ratios. The patchwork shields are shown in Fig. 3. On completion of the fabrication processes, the shields were cemented to the relay in a position shown in Fig. 4. This position varies with the type and manufacture of the relay.

The reduction of radiated magnetic field from a magnetic latching relay for each of the patchwork shield pairs is tabulated in Table 3. Note that shields number 7 and 8 produce the greatest reduction of radiated magnetic field. This particular relay and shield combination is shown in Fig. 5.

Electrical tests were performed using shields numbered 1 through 9 on the same relays. In all cases, a reduction in relay average trip current was observed. The percent reduction is tabulated in Table 3. Six relays, all the same magnetic latching type as used previously, were shielded with the number 8 configuration shield and electrically evaluated in staggered back to back and flat side by side array. In the staggered back to back array recommended by the relay manufacturer, but with the patchwork shields, an average trip current reduction of 18.8% was observed. Magnetic shields of the patchwork type have survived, without degradation, all dynamic environments that the relays are expected to survive. It is anticipated that this type of shield will be recommended for use on relays aboard later interplanetary spacecraft.

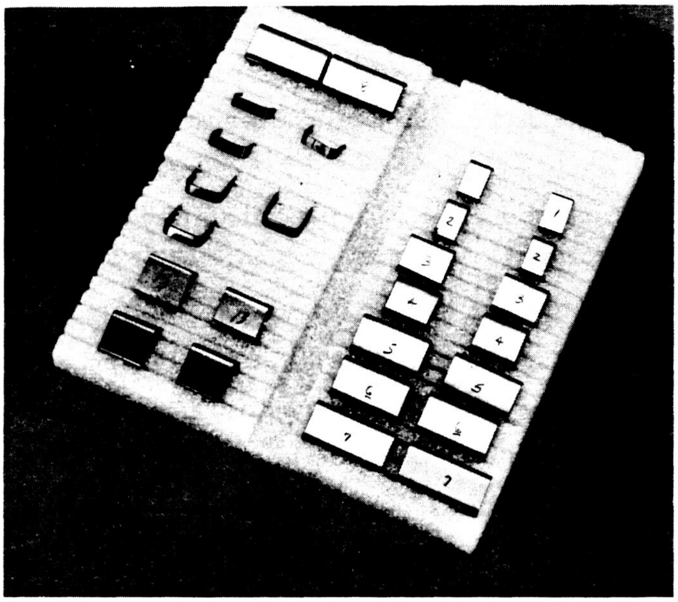


Fig. 3. Patchwork relay shields

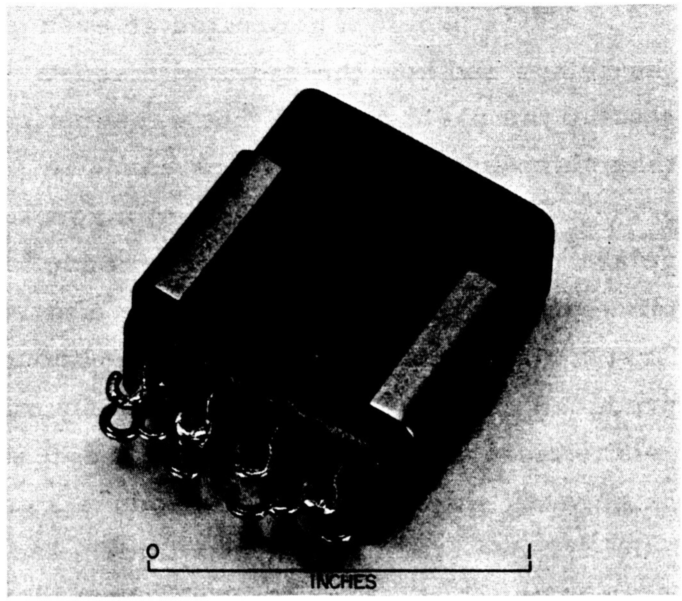


Fig. 4. Latching relay and attached typical patchwork shields

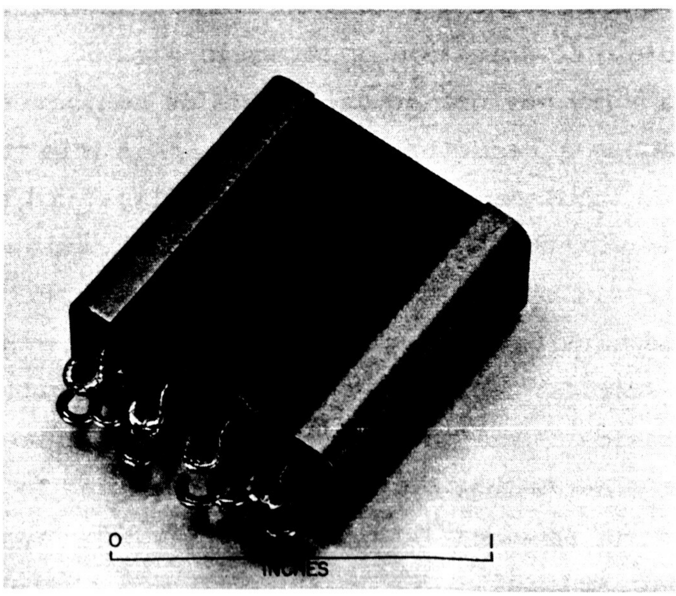


Fig. 5. Latching relay with best patchwork shields attached

Table 3. Patchwork shield evaluations

	Shield number	Field, gamma at 18 in.	Shielding factor	Trip current reduction
Relay only	-	40.9	-	-
Relay and shield	1	25.8	1.6	7.7
Relay and shield	2	25.8	1.6	7.7
Relay and shield	3	18.6	2.2	17.0
Relay and shield	4	15.6	2.6	17.0
Relay and shield	5	7.5	5.4	12.2
Relay and shield	6	9.0	4.5	12.2
Relay and shield	7	4.5	9.1	12.2
Relay and shield	8	4.5	9.1	12.2
Relay and shield	9	35.1	1.2	9.1
Relay and shield	10	39.3	1.0+	-
Relay and shield	11	38.2	1.1	-
Relay and shield	12	40.1	1.0	-
Relay and shield	13	22.2	1.8	-
Relay and shield	14	20.0	2.0	-

Odd shield numbers: 0.014 in. moly-permalloy
Even shield numbers: 0.025 in. moly-permalloy
Average relay trip current = 6.5 ma

MOTOR DRIVEN POWER SWITCH

The strongest single magnetic source in the Mariner Mars spacecraft was the motor driven power switch. The power switch is a part of the power regulator subassembly. There is one power switch per spacecraft. The Mariner Mars spacecraft power regulator subassemblies were magnetically evaluated with field strengths given in Table 4.

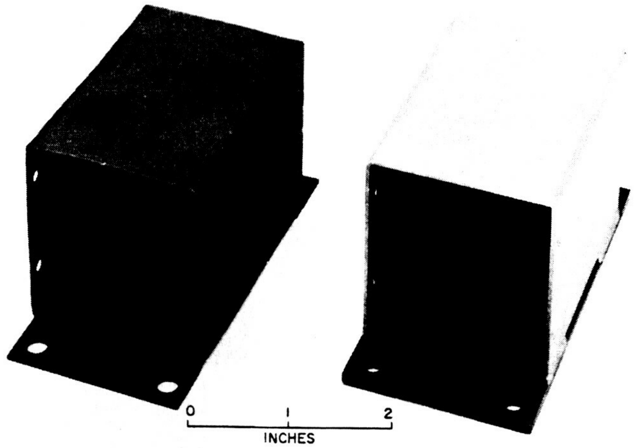
Table 4. Power r gulator magnetic evaluations

Serial number	Date	Field, gamma at 3 ft	Status
JPL001 PTM	10/10/63	153	After flight acceptance shake
01	11/26/63	100	After flight acceptance shake
02 (TA)	12/17/63	95	Before type approval shake

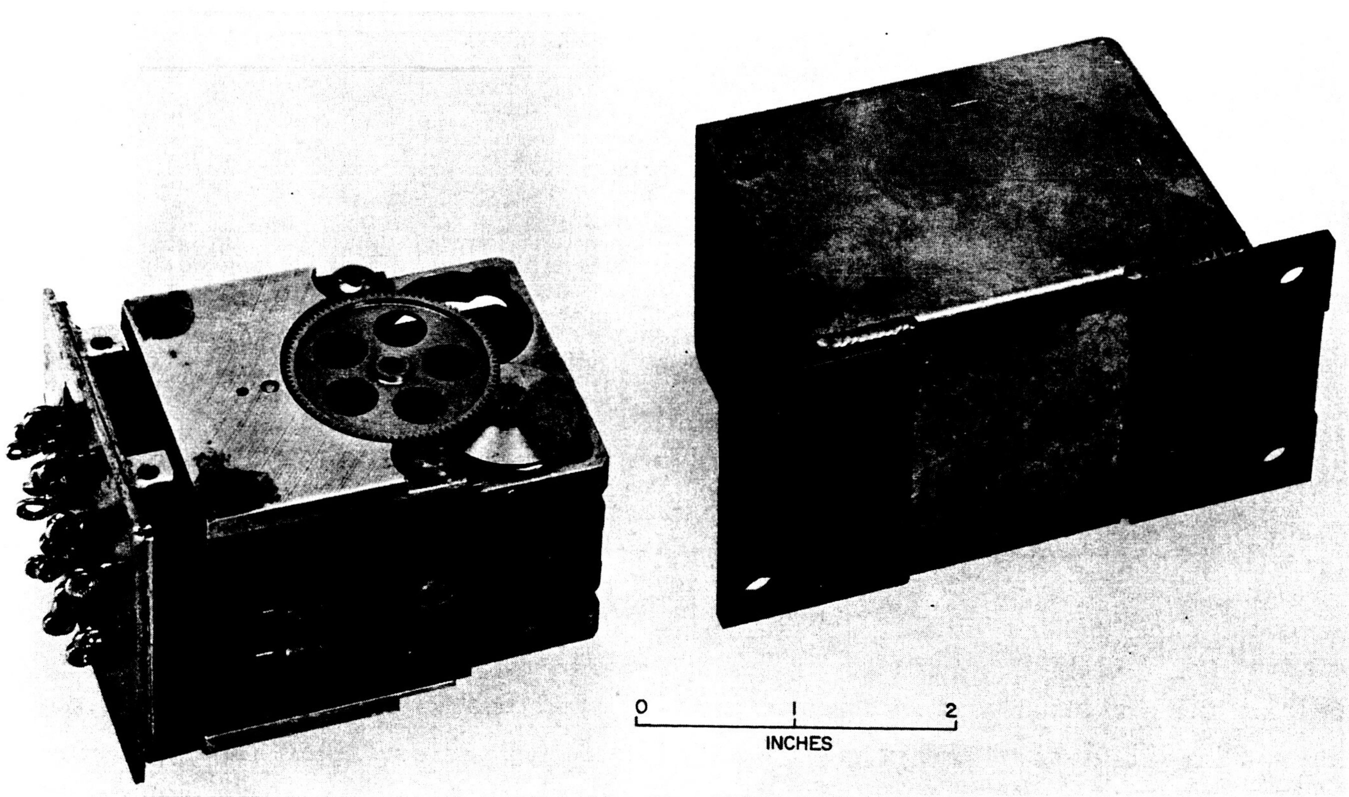
During the above evaluations it was noticed that most of the field strength was caused by the motor driven switch. To reduce the radiated magnetic field of the motor driven switch, Mumetal shield boxes of several geometric configurations were constructed and magnetically evaluated. The initial shield was a 0.025 in. Mumetal box. The motor driven switch around which this shield was formulated had a magnetic field of 123 gamma at 3 ft. The above shield reduced this field to 0.4 gamma at 3 ft before and 0.7 gamma at 3 ft after vibration testing. Thus, a shielding factor of 176 was obtained from the initial shielding attempt. Because the shield was also the mounting interface, structural dynamics requirements necessitated a thicker walled box shield on a stiff base plate. The initial shield and the flight configuration magnetic shield for the motor driven switch are shown in Fig. 6. The flight configuration shield is made from 0.050 in. Mumetal. It is welded to an S. A. E. 1020 steel base before hydrogen annealing.

The flight accepted motor driven switches produced less than 1 gamma field at 18 in. after flight acceptance vibration tests. The results of this shielding experiment were a weight increase to the spacecraft of 6.6 oz, and a reduction of the magnetic field at the magnetometer sensor location of about 15 gamma. This magnetic field reduction also brought the power regulator subassembly within the Mariner project magnetic restraint of 5 gamma at 3 ft.

Although shields of 0.050 in. Mumetal were used on the flight motor driven switches in the Mariner Mars spacecraft, moly-permalloy shields of the same configuration have been made and evaluated under the specified dynamic environments of shake, shock, thermal-vacuum, and high magnetic fields. The moly-permalloy shields were slightly more stable than those of Mumetal, as would be expected.



a. Initial shield on left and flight configuration shield on right



b. Flight configuration shield with motor driven power switch

Fig. 6. Motor driven power switch magnetic shields

RF CIRCULATOR SWITCHES

During May 1963, serious question was raised as to whether the Mariner Mars RF circulator switches would meet the project magnetic requirements of not only low magnetic field but also a stability of 10% or less. Therefore, a series of evaluations were performed on one junction of the four port circulator switch, shown in Fig. 7, both with and without electrical power and with and without the manufacturers "netic" C strap (three-sided) shield. Table 5 presents the data from these early evaluations.

Table 5. Prototype Mariner Mars four port circulator switch

Condition	Current, ma	Field Strength, gamma at 18 in.		
		Axis 1	Axis 2	Axis 3
No power				
No shield		18	-	-
Three-sided netic shield on	0	6	-	-
Power on - A polarity				
No shield	13	65	-	-
Three-sided netic shield on	13	48	-	-
Power on - B polarity				
No shield	11	56	-	-
No shield	13	59	-	-
No shield	15	65	-	-

The A and B polarities (referred to in Table 5) are an arbitrary convention to indicate the direction of current flow in the circulator switch solenoids and, later, indicate the direction of the magnetic fields. From the Table 5 data it would appear that the manufacturers shield had a low permeability and contained some remanant magnetism. These tests determined that better shielding, if available, was highly desirable.

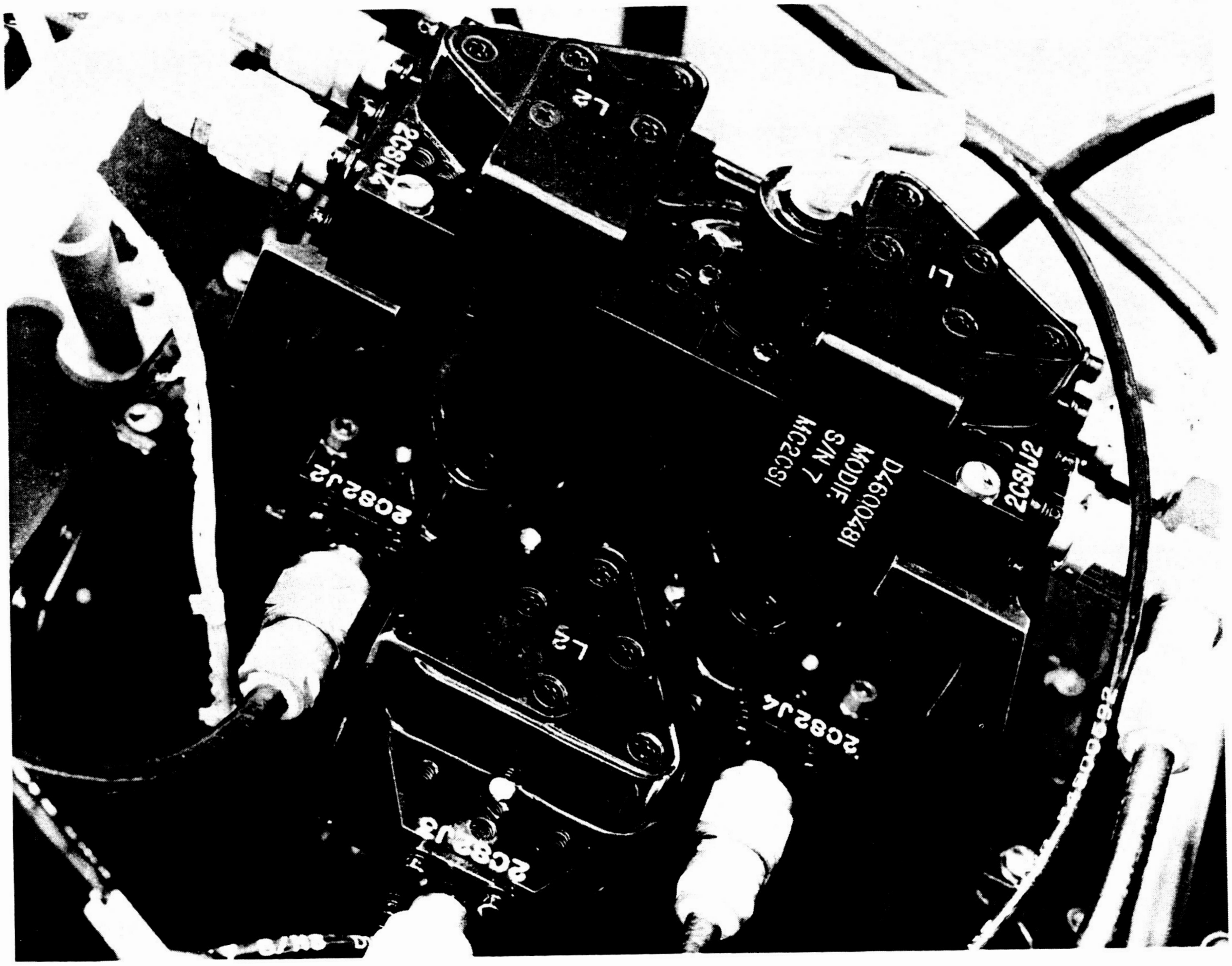


Fig. 7. Mariner Mars RF circulator switches

After discussions with the manufacturer about the magnetic circuit within the circulator switch, two magnetic shield configurations were made. The first of these was a three-sided C strap made of 0.050 in. Mumetal and duplicating the geometry of the original shield. The second shield was a four-sided rectangular strap of 0.050 in. Mumetal with a welded butt joint, Mumetal filled. Subassembly weight limitations time and the geometric envelope of the circulator switch precluded a more comprehensive effort of magnetic shield design. These shields are shown in Fig. 8. The data from the magnetic evaluation of these shields is listed in Table 6.

Table 6. Prototype Mariner Mars four port circulator switch

Condition	Current, ma	Field strength, gamma at 18 in.				Shielding factor
		Axis 1	Axis 2	Axis 3	Total field	
No power	0	0.5	0	5.2	5.2	
Four-sided Mumetal shield	0	2	1	6	6.4	
Power on - A polarity						
No shield	13	22	2	4	22.4	0
No shield	16	24	2	4	22.4	0
Three-sided Mumetal shield	13	4	3	6	7.8	2.9
Three-sided Mumetal shield	16	4	3	6	7.8	3.1
Four-sided Mumetal shield	13	3	2	6	7.0	3.2
Four-sided Mumetal shield	16	3	3	6	7.4	3.3
Power on - B polarity						
No shield	13	19	1	7	20.3	0
No shield	16	22	1	7	23.1	0
Three-sided Mumetal shield	13	2	1	5	5.5	3.7
Three-sided Mumetal shield	16	2	1	6	6.4	3.6
Four-sided Mumetal shield	13	2	0	5.5	5.9	3.5
Four-sided Mumetal shield	16	2	0	6	6.3	3.7

Although the magnetic field reduction provided by these Mumetal shields brought the total magnetic field for one operating junction of the circulator switch within the specified design restraint of 5 gamma at 3 ft, a quasistatic field change

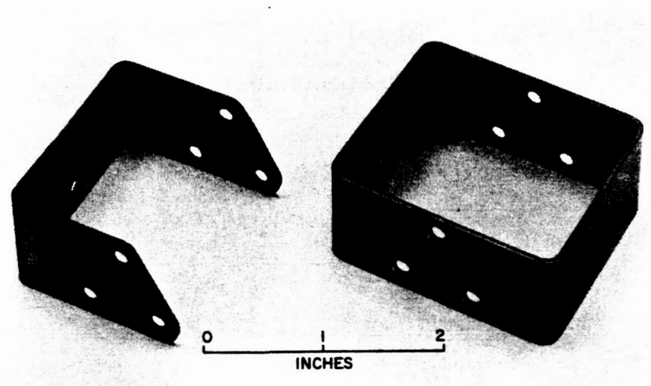


Fig. 8. RF circulator switch magnetic shields

because of polarity reversal of as much as 20% was still present. Also, an average shielding factor of only 3.4 was obtained. This low shielding factor, which probably also accounts for the instability on polarity reversal, was because the Mumetal shields were moderately cold worked. This cold working was the result of slight deformation of the shield when installed on the circulator switch. It was later determined that the Mumetal sheet stock, from which the shield was made, was not fully annealed. The shield then shrank about 8% on annealing. In more recent shielding experiments, where fully annealed material stock was used for fabrication, no shrinkage has occurred during the annealing cycle.

Time schedules prohibited the magnetic evaluation of addition shielding configurations and each Mariner Mars flight circulator switch as a single entity. Routine subassembly magnetic mapping, however, of the radio subsystem (of which the circulator switches are a component) definitely indicated that the three-sided Mumetal shields used on the flight circulator switches were performing as well as those used in the initial shielding experiment. The result of this shielding experiment was to comply with the project design restraints for total magnetic field without system performance and weight penalties.

REMARKS

In conclusion, several gross observations are appropriate.

1. High initial permeability materials of the types of Mumetal and moly-permalloy may be satisfactorily used to magnetically shield or contain the magnetic field of hard magnetic sources aboard contemporary spacecraft. Some field exists at the magnetometer sensor location of Mariner IV because of the spacecraft itself. If the motor driven power switch and the RF circulator switches had not been magnetically shielded, the spacecraft magnetic field contribution at the magnetometer sensor location would have been three to five times greater.
2. Shielding, however, is not a panacea and should be used as a last resort. Soft magnetic sources such as transistor cans, high nickel alloy electronic header leads, and welded nickel ribbon modules cannot, in general, be satisfactorily shielded. Occasionally pairing and preferential placement are better magnetic

problem solutions. When hard magnetic sources must be incorporated into a system, shielding may be a good remedial approach.

3. Engineering common sense in the design, fabrication, and handling of a magnetic shield is essential to a successful shielding effort. As outlined at the beginning of this paper, the shielding philosophy must be simple, the source known, and the approach systematic.
4. Only a fully annealed magnetic shield can be significantly effective at low field levels. Annealed shields must be handled with care because they are readily degraded by cold working.

While shielding is, at best, a remedial measure, it is anticipated that the electromagnetic devices on complex future spacecraft will necessitate appreciable shielding efforts. Further development of shielding technique is vital to reduce spacecraft magnetic fields.

OPEN DISCUSSION

MR. JANSEN: Nick Jansen, Motorola, Scottsdale. I am curious if weight were the limitation on not fabricating the circulator out of Mumetal because circulators do lend themselves to a completely closed path by containing the magnetic field inside the metallic structure?

MR. CHRISTY: You are suggesting that, possibly, the body of the circulator switch could have been made of Mumetal?

MR. JANSEN: Right, unless weight were the prime factor that would eliminate this.

MR. CHRISTY: Originally, the body was, I believe, Armco iron and weight determined that they would go to aluminum. The body of the circulator switches as flown on Mariner IV were, indeed, aluminum. Weight in this case was the criteria, yes.

MR. JANSEN: Is the shielding you have on there the most efficient for trying to contain it in an aluminum body?

MR. CHRISTY: This was an after-the-fact experiment, unfortunately.

MR. JANSEN: It turns out it is at least as effective in a ratio of 40:1 in some of the studies we have been doing on this sort of thing.

What sort of RF power was involved?

MR. CHRISTY: That I can't answer; I don't know.

MR. JANSEN: It would be an academic point at this time, because we can always look back in hindsight at this point, and because (in 1965) there are high reliability semiconductor p. i. n. switching diodes. Some figures I have show that the mean time between failures are somewhere around 300,000 hr, and this is only a point of where the reverse current starts to increase. It doesn't actually represent a failure of an RF switch.

The other thing is that here the RF engineer is grinding an axe in the experimenter's favor because, unlike low frequency transistors, the RF engineer says, "You will not put a can over that thing because it adds case conductance." The other factor entering into it is that the RF engineer doesn't like any lead length. So this is something that we can certainly look into for future spacecraft.

CHAIRMAN GAUGLER: I think the power involved is about 5 w. Isn't that what the transmitter is on Mariner?

MR. CHRISTY: The transmitter is, I believe, 10 w. These power levels are very reasonable for this type of diode.

CHAIRMAN GAUGLER: Well, there is another problem on the circulator; that is, there wasn't enough attenuation.

MR. JANSEN: This you can take care of very well by operating the two in conjunction. Reliability engineers will come chopping at me with an axe, but there are ways of looking at this thing in terms of the switch, looking at a reactive load. Then I say, "All right, let's put the circulator in there as an isolator and terminate its third port." But, here, when we put a strap around there and reduce its field, it is now a static element and not a dynamic element that is switching every time you flip the current in the coil.

CHAIRMAN GAUGLER: Did you say they made circulators out of Mumetal?

MR. JANSEN: This has been done, at least out of soft irons, this has been done to contain the magnetic field in the vicinity of the circulator, rather than have a lot of leakage. This was one advantage gained immediately in going from resonance-type isolators and coaxial line at these frequencies to strip line

circulators; the coaxial isolators required very heavy magnetic shielding cans. Normally, it blew the size of the package up a factor of three to four. Basically because it required a bigger magnet and you had to put a bigger shield around it.

MR. PARSONS: I just had a few additional pieces of data. I brought a lot of material with me, but I can't locate it all fast enough.

This one is one on a velocity selector that is aboard the IMP F, or is proposed to be aboard the IMP F, satellite. This was shielded with a material I am sure you are familiar with; the conetic material laid on in foil and then a spacer foil of nonmagnetic material, then a second layer and a third layer. I am just running down the numbers. At 18 in. this device had an initial perm problem of 105 gamma. This was cut to 18 gamma with the first shield, to 10 gamma with the second shield, and 8.8 gamma with the third shield. At that third shield condition it was reexposed, again, and exposure puts it up to 54 gamma.

The exposure without any shields was 160 gamma. The post deperm field, however, is 8 or 9 gamma for an original starting problem of 105 gamma. So, 90% down, perhaps.

MR. CHRISTY: Do you have any stability information on that?

MR. PARSONS: No, I don't.

CHAIRMAN GAUGLER: One factor that people tend to forget when they design shields – remember Ben Connors had this formula this morning showing the effective shielding factor as a ratio of the thickness of the shield to some dimension of the shield. Now, if you take his room up there, if you have a 50-mil thick metal with a 7-ft diameter room, for example, that's a pretty low ratio. Then, when you make a shield for a relay you might use the same thickness of material, but your dimensions are only about that big. Many times you can get away without annealing it or even using this conetic stuff (which is not real high quality, as far as – if you made a ring core out of it, it wouldn't be very good).

Most shields for relays are designed for mechanical stability. They could use 0.0001 material or 0.005 material, but it would deform, so they make it 0.0002 or 0.0004 for mechanical rigidity. So you have a lot more material than you really need.

MR. CHRISTY: The point I am getting to, I guess, is that our experience has been that only the 4-79 class of materials; namely moly-permalloy, have really shown any magnetic stability under any sort of reasonable environment you want to

submit it to. Where, some of the others, including that which you have mentioned, is subjected to all sorts of things.

MR. PARSONS: We just recently were experimenting with a new material. Probably you are familiar with it. It is called shield mu tape and foil. It is put out by a different company. It is 80% nickel-iron alloy, and 20% of various combinations. I have no data on this yet, except that the experimenter believes this to be a better material.

MR. CHRISTY: Just for information's sake, I am acquainted with it, and it is basically a good-grade Mumetal.

MR. PARSONS: One other thing on the subject of relay problems; we have worked a little bit with matching pairs and matching combinations, and all this. I would just mention one here.

The peak field of a single relay was on the order of 44 and, after matching with a similar relay back-to-back style, this dropped to 3.7. A similar problem was attempted with an array of 40 latching relays, and the result was not so encouraging. In fact, I am reluctant at the moment to say what the final result was. The experimenter went away discouraged.

MR. CHRISTY: If you have had the same experience that we have had with variation in the field before and after energizing the relay, and things of this sort, I well understand your hesitancy there.

MR. LAMPSON: My question may be out of place here, but, frankly, I am kind of confused. The first day I was here Mr. Casani, I believe it was, said that if you tinplate or electroless-nickelplate something, that you disturb the magnetic shielding efficiency. Now, you talked about Mumetal here today, and I am constantly having questions asked me about the corrosion resistance of Mumetal. It is not a corrosion-resistant product; it will corrode. Yet I know that people do tinplate it, they electroless-nickelplate it. What I am really building up to asking you is, on the particular Mumetal shields that you showed on your slide, were they protected from corrosion?

MR. CHRISTY: No, we made no attempt to protect them in that manner. In terms of the relay shields, now, the motor-driven power switch has, I believe, a passivation process imposed on it. Don't quote me, I am pulling that off the top of my head. But I know, as was obvious from the photograph on the circulator switch,

that they were gooped up something horrible. I don't know what all was on them: paint, epoxy and a few other things? So they had a very definite corrosion requirement on the subsystem and, of course, the shield would also have to meet that requirement.

But again, I don't know exactly what.

CHAIRMAN GAUGLER: Wasn't Mr. Casani talking about providing a base for soldering, I think he said some people nickelplate, and then you get a magnetic moment because of the nickel?

MR. CHRISTY: Yes, I think this is what Mr. Casani had reference to yesterday. Now I would point out, because you bring up this electroless-nickel, we have one data point that is the omni-antenna wave guide. Electroless-nickel is used there to form a base for whatever goes on top of it, gold, silver or whatever it is.

As a matter of fact, we got into a problem there. We had a 7-ft section of this 4-ft wave guide. Not only couldn't we measure a field from the electroless-nickel that was internal to the tube, but waving this 7-ft tube in the Earth's field we couldn't even see an induced moment in it, and we didn't know whether we had some real oddball nickel, whether it was an experimental goof, or what.

MR. IUFER: Just a comment on electroless-nickel. STL and a couple of the experimenters have done some studies for us on electrical nickel. If you have extremely careful process control, you can lay out electroless-nickel that is non-magnetic; however, we have pretty well abandoned any use of electroless-nickel because of the uncertainties in the process control.

The problem is precipitation hardening (PH) and the heat treatment to prevent hydrogen embrittlement. Both PH and this heat treatment seems to adjust the structure of the nickel laid out, so that it becomes a magnet.

MR. CHRISTY: We were lucky.

CHAIRMAN GAUGLER: In fact, if it becomes thin enough, it will be a darn good permanent magnet.

MR. REILLY: Frank Reilly from Texas Instruments.

We are using electroless-nickel on magnetometers that have sensitivities greater than 0.1 gamma, and we haven't had any problems.

MR. IUFER: How do you measure its contribution at the magnetometer if they are both fixed?

MR. REILLY: We have magnetic check techniques, where we check everything that goes in the sensor. We are concerned with maneuver signals from the magnetometer. Everything on the sensor must be clean.

MR. GOLDSTEIN: We can also monitor the frequency corresponding to the magnetic field and get an absolute measure that way – monitor on a counter. In fact, we have used electroless-nickel to form a faraday shield on the magnetometer head itself.

MR. WEINBERGER: Do you have any information on the degradation of shielding with radiation fields as well as its temperature susceptibility?

MR. CHRISTY: Temperature susceptibility is pretty much straightforward. This is a property of the material, basically; the configuration doesn't enter in. As such, I think this is pretty well established. Radiation, I don't know.

CHAIRMAN GAUGLER: Dan Gordon will be here tomorrow. He's from the Naval Ordnance Laboratory (NOL) and he's done a lot of work on taking Mumetal and moly-permalloy and putting it into almost atomic bombs. He can give you quite a curve on what the degradation is due to radiation. I think, in general, the higher the permeability and lower the coercive force, the more susceptible it is. But I think that it takes a fairly substantial number of rads to do something, though.

MR. WEINBERGER: As far as temperature is concerned, I was given to understand that there is a possibility of selection of Mumetal that somehow will reduce its temperature sensitivity. In other words, more than the theory would indicate. Are you aware of this, or do you have any comments?

MR. CHRISTY: I am aware that there are different grades of the various shielding materials. This is another hazard that I didn't mention, really. There is a variety of grades of Mumetal, moly-permalloy, or some of the other materials that fall in the same class. I know that there is a variation in the temperature dependence of these materials.

CHAIRMAN GAUGLER: I think if you bring that question up tomorrow, when we talk about materials, these people from NOL will be here and they can tell you about it. A lot has to do with the Curie point of the material and, also, at what level you are operating. For example, as the temperature goes up, the initial permeability actually increases until you get to the Curie point, and then all of a sudden it goes to zero.

Now, if you are up above the knee of the curve, for example, saturation tends to go down like a parabola to zero at the Curie point. There is a place where it is almost constant. It depends on where you are operating. I wish that you would bring this up tomorrow when the NOLers are here and they can go into this.

MR. CHRISTY: It is also a function of the anneal cycles, and temperature slopes. It is also a function of the contaminants in the atmosphere of the annealing oven. It is kind of involved, really. The general effect isn't that great, typically.

MR. WEINBERGER: I heard 42%; this bothers me. By selection of Mumetal, not a change from moly-permalloy to Mumetal, but by a selection of Mumetal. Whether this was from manufacturer to manufacturer, the figure from a reputable source said that he could reduce the temperature coefficient – and I've forgotten the temperature range – from 42% down to something like 5%. So it seems something to be concerned about.

MR. CHRISTY: We have never seen anything greater than 8 to 10%.

CHAIRMAN GAUGLER: I wonder if you might be getting into thermal stress there.

MR. WEINBERGER: It was not my impression. I got the impression it was, somehow, something black magic.

MR. CHAIRMAN GAUGLER: I wish you would bring that up tomorrow, because these fellows work with this all the time and they could certainly clear that up.

MR. LEDBETTER: Ledbetter, General Motors. Mr. Christy, you mentioned that on your motor-driven switch you used the shielding as a structural casing for that hermetically-sealed motor switch. How did you achieve the hermetic seal?

MR. CHRISTY: I didn't. Actually, all I did was provide the basic concept for the shield. The fabricator of the subassembly, whose responsibility is the switch and its subsequent shielding, undertook that with the switch manufacturer.

MR. LEDBETTER: When they made the hermetic seal, they did it without introducing local stresses in the shielding material?

MR. CHRISTY: I doubt it, but the local stresses were, apparently, not significant. The flight units, as I mentioned, were quite good.

MR. GRUMET: I have some suggestions that might reduce weight if multilayer shields are used. We have made some digital computer runs on exact solutions for spherical geometry; we find there is a maximum. By increasing the spacing, you can go through a rather broad maximum; which indicates that if you reduce the gauge of the shield and introduce two or three shields, you might get the same or better shielding and have a reduction to boot.

Unfortunately, the best spacing is about the radius of the innermost shell. But, because the maximum is broad, you need not go to the best and still realize an improvement factor of two or three.

MR. CHRISTY: I appreciate that. We have considered multiple-shell shields and, unfortunately, in some of this we haven't progressed to the diesel locomotive area, we are still in the steam engines.

MR. PEIZER: This is a comment on the source in the motor-driven switch. We have found that the motors normally supplied, that is, the small DC motors, are 2-pole. These have a much larger field than the four-pole models.

MR. CHRISTY: The armature on this was something like five poles, some odd configuration, anyway. We found, late in the game, that the manufacturer changed motors on us about three times; and I, myself, am not sure to this day exactly what we flew. It was shielded, that's all I know.

MR. PEIZER: In any case, that is a good guide for future motors; to get an even number of poles and fairly balanced.

MR. NOBLES: I think that this is a question that should have been asked earlier, but has anyone considered the effect of the magnetization by rotation — I believe it is called the Einstein-de Haas effect — in the spacecraft; where they are rotated about the axis, you can't sense this magnetization.

The second part is that intuitively I would expect this to be worse with soft materials, and I wonder if this effect does occur if this might be a reason for not using shielding materials? I don't think this has been considered. I wouldn't attempt to say why or why not.

MR. CONNOR: By way of background, the Mariner Venus circulators used permanent magnets. In the breadboard form, I remember, I went down to look at these magnets and was thrown out of the laboratory; and I came right back in and

I looked at the field with a gaussmeter and extrapolating out to say 5 ft to the magnetometer location, the field contributed to the magnetometer would be on the order of 1500 to 3000 gamma and the magnetometer had a full scale of 128 gamma. I think that the work done on this is an illustration of what shielding can do. We talked to the manufacturer and persuaded him to run some experiments. By the time he got through, he not only had the field down to about 6 gamma at the magnetometer, but he had actually reduced the weight of the subassembly by having a more efficient magnetic circuit.

MAGNETIC SHIELDING EXPERIENCES

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N66-11293

I am the cognizant engineer on the Mariner Mars magnetometer. I would like to discuss some of the experiences that Larry Simmons and myself have had with both shielding the magnetometer for testing and component shielding. They are really two distinct topics. Try not to get lost halfway through when I switch gears.

First of all, I would like to discuss the two shields that were developed for the evaluation of the Mariner Mars magnetometer sensor. Figure 1 is a picture of the magnetometer itself; to give you an idea of its size, a 6-inch rule is in front of the sensor. The instrument itself was built for JPL by Texas Instruments (T. I.). The project engineer from T. I. is here, Mr. Frank Riley. Incidentally, this picture is through the courtesy of T. I., also.

The standard JPL chassis that people have been talking about is the 6- by 6-inch chassis, shown by the sensor. The depth of the chassis varies from one unit to another. The photograph shows the magnetometer sensor in the center. The shields that I am going to talk about were designed to house this sensor. The spherical portion of the sensor is a Helmholtz coil system used for nulling the field at the sensor. The sphere is 4 in. in diameter.

The first shield, shown in Fig. 2, is a shield that was built to test the instrument in a vacuum. Because it was used in a vacuum environment, there are some extremely bad things (from a shielding standpoint) that were done to it. The outside diameter of the cylinder is 24 in., and the inside is 22 in. It is a double shield, and the inside shield is separated 2 in. from the outside shield. There are several places for lids. There is a lid central closure to allow for two separate shielded compartments. Because this is used inside of a vacuum chamber, we wanted a fairly rugged mechanical unit, so, several things were done to ensure mechanical stability. The manufacturer, Williams Manufacturing Co. in San Jose, California, recommended that the walls of the shield be a double layer of 0.030. It will tolerate more handling that way; it is more flexible without deforming. Because it is used in a vacuum, we wanted to eliminate stresses so the two-layer shield was built with Mumetal braces holding the inside shield to the outside shield. Everyone seemed to think that was nonsense because, obviously, you are shunting your shield path, but it seemed to work out all right. The thing that was amusing was that it had 1/2-in.

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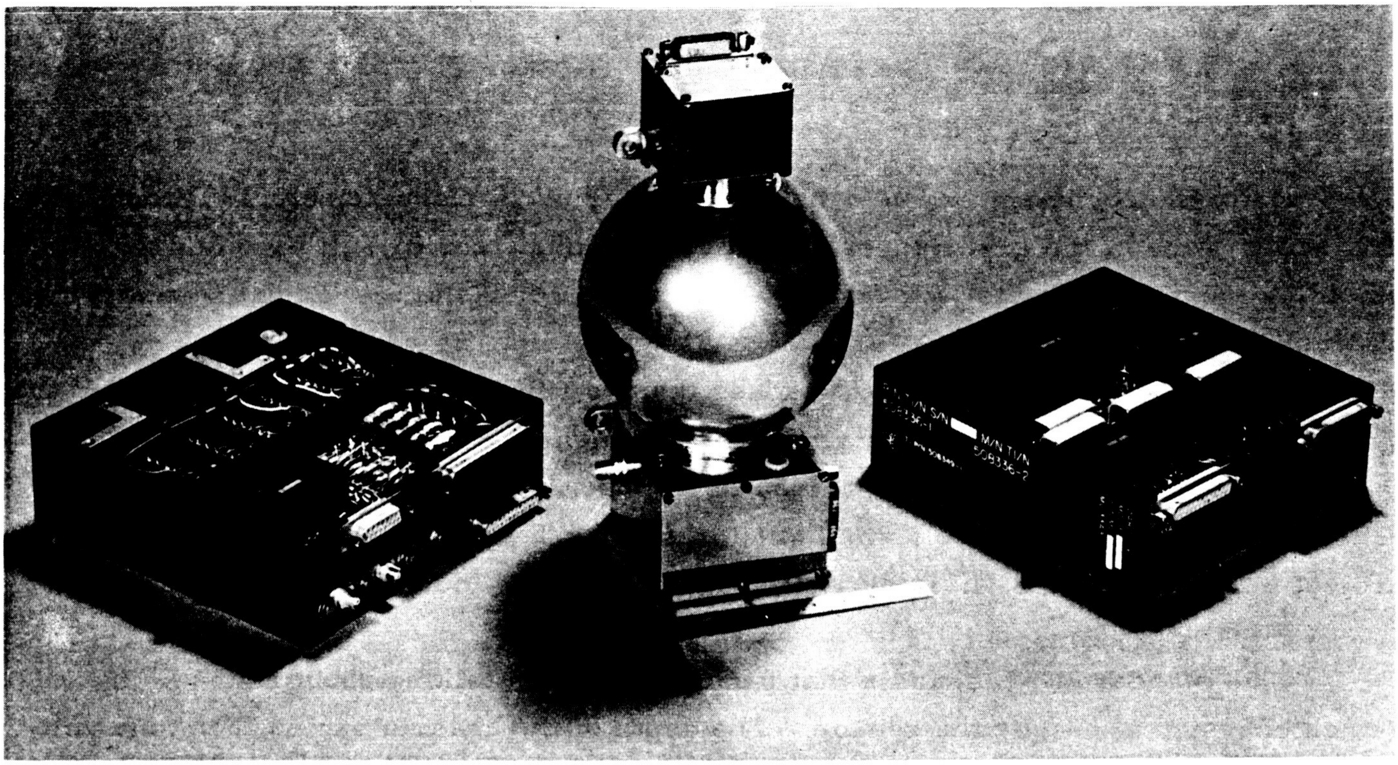

Fig. 1. Mariner Mars magnetometer and magnetometer electronics

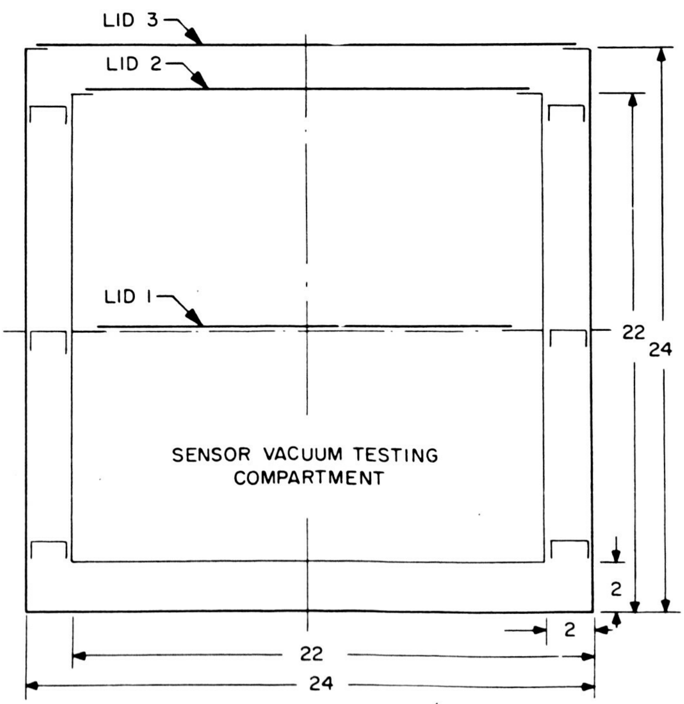

Fig. 2. Magnetic shield for thermal-vacuum testing

holes drilled all over it and around it, to make sure that the gas had some place to go.

The magnetometer sensor sits on a heat exchanger inside one compartment. Gas lines come in through the lid and we control the temperature of the magnetometer sensor in the vacuum from +55 to -65° C.

The shielding factor is about 1600. The field inside is roughly 30 gamma at the magnetometer location, and it has been that way for the past 1-1/2 yr. So, it seems to be fairly stable. We have had the chamber in two different places, but it turns out that the shield has had the same relative orientation to the geomagnetic field both times. Actually, it is sitting in possibly a better position because the field is coming through the side of the cylinder and not down the end, which might not be quite so satisfactory.

There is another shield, developed for testing the magnetometer sensor, and shown in Fig. 3, that is considerably larger than the first one. The dimensions of this shield are that the height of the outside shield is 42 in., and it has a diameter of 30 in. The inside shield is 30 in. high and has a diameter of 18 in. These are again, both coaxial cylinders. The inner shield sits on a wood platform, so it is mechanically independent of the outer shield. There is a wood platform with just a single screw holding it in place so it has minimum mechanical strain on it.

The design goals for this tank were quite different from the other. This one is used to bench test and calibrate the magnetometers. Actually, the primary calibration was done in the magnetically-shielded room on the Mesa. This large shield was used at the Cape for final verification. So, the inside shield is large enough to allow for a flipping of the sensor.

The sensor sits on a fixture at the center of the inner shield, which we can rotate, and allows a check on the instrument offset to make sure it hasn't been contaminated with a magnetic screwdriver or other magnetic tools. The field at the test point is 14 gamma, which is a shielding factor of 3600. Primarily, because we have finite air gaps in these lids, we can't really achieve the ultimate shielding factor. The incremental shielding factor is 2000; which means that if a truck drives by, the field because of the truck (or whatever you have), will be attenuated by a factor of 2000. That is actually how we found the incremental shielding factor, by having one magnetometer inside the tank and one outside the tank, and waiting for a truck to go by.

We had to know when the truck came by because it was hard to find the truck inside the tank (fortunately) so, we had to know which part of the noise to pull it out of. You get a truck driving 10 ft away, or a bus — I guess the buses are the worst,

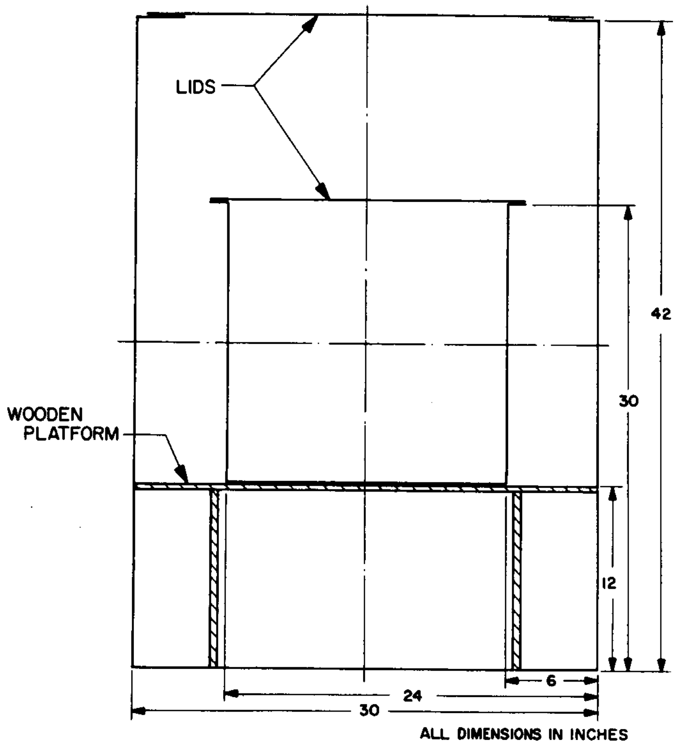

Fig. 3. Magnetic shield for bench testing and calibration

the field inside varies about 0. 1 gamma. This shield has the nice property that you can work in a laboratory; you can do a considerable amount of testing with a magnetometer, without really going to great pains to keep everyone out and to hold down the moving of equipment.

Another important factor about this shield is that it has stability over 12 to 16 hr. Something varied 0. 1 gamma over a 16-hr period. Now, this was in a quasi-regulated temperature environment. We live in trailers, and they are not terribly good in that respect. The temperature, however, did not vary more than 10° F over that period of time.

So, with that variation, the shielding factor was stable to the point where we saw only about 0. 1 gamma variation. That might have been our recorder paper moving around — I am not sure.

I would entertain any questions that you might have on the shields because I am going to break into another topic.

OPEN DISCUSSION

MR. IUFER: Did you find this method of providing an environment for magnetometer checking satisfactory, or do you recommend it?

MR. NORRIS: By satisfactory, I have to qualify that. This is the first time that I know that this has been done; testing a magnetometer sensor in a low field environment, in vacuum, over temperature. We are not able to check offsets in a vacuum. We did not have a fixture where we could flip the thing over. It was quite satisfactory from the standpoint that the instrument noise wasn't vastly greater because of the presence of vacuum pumps, etc. We were able to do fairly extensive evaluations of the instrument's performance inside the vacuum.

MR. IUFER: Was the gradient inside the can significant when you made the DC offset test; did you flip the transducer about its magnetic center?

MR. NORRIS: When we first got it and checked it, the field was about 3 gamma higher at the wall than it was at the center. So, we didn't have a terribly high gradient; however, after it was shipped down to the Cape and back, the gradient had increased.

MR. IUFER: Does the gradient introduce error when you measure the DC offset?

MR. NORRIS: Only in the sense that you might move about the center, the actual geometrical displacement. Yes, it would, and I don't think we have our center pinned down that tightly where we can say exactly where the center of the measuring system is. The only convincing argument I can give is that we got exactly the same results in the tank as we got in the shielded room.

MR. PARSONS: The vacuum chamber you were in, in the steel tank, what was the gradient in the tank before you came in with the shield?

MR. NORRIS: We looked at it, and it wasn't terribly exciting. It was a stainless steel tank, so it wasn't iron in that sense. Stainless steel is bad for flight hardware, but it is not too bad for the real world.

MR. PARSONS: What was the thickness of the shield on the second design?

MR. NORRIS: The outside shield on the large one is a double layer of 0.030, and the inside shield was 0.050, single-layer. All by itself, the inside shield had a shielding factor of about 300 to 600 when taken out of the inside tank. Actually, what we used to null the inside field down with, was to have a coil on the outside of the inside shield. It turned out to be a little fakey, though, because it wasn't directly proportional and there was a skew angle introduced because of the shield, but one could tweak it to get zero field, and it would stay there. I don't know whether that was clever or that we just did it that way and it worked accidentally. It was quite stable in that mode.

COMPONENT SHIELDING

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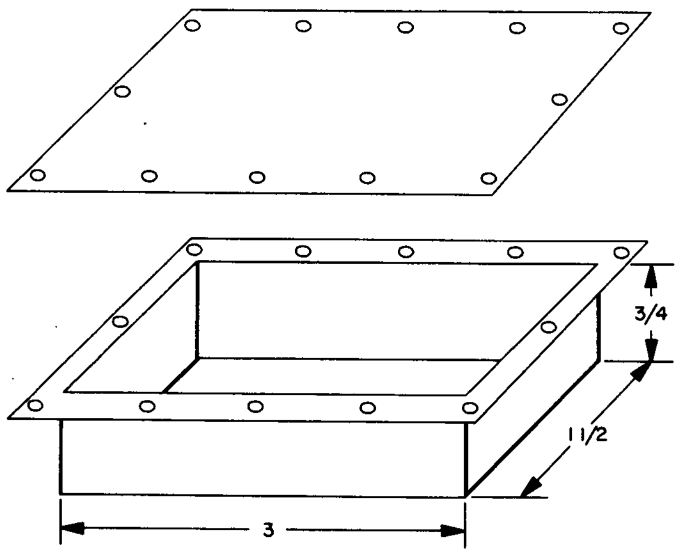
My next topic is on component shielding. Actually, we only have one little experience here with shielding. The object was to shield reed switch relays. I am not sure that everyone is familiar with reed switches.

A reed switch is two small reeds of low reluctance magnetic material. It is inside a glass envelope. By generating a field in one direction, it induces little magnets. The two poles are then closed because of the magnetic force between the two little magnets. The field to close them has to be something in the order of 20-amp turns. So, there is a coil around them that generates a fairly healthy magnetic field, but not quite as bad as standard relays, such as latching relays.

Nevertheless, they are magnetic because of the current-loop magnetic field. Also, the leads are magnetic. So, the shielding that we did work on was for these reed switch relays.

The small shield that we built, and shown in Fig. 1, is 3/4 in. deep, 1-1/2 in. wide, and 2 in. in length. It has a lid that is just a lid. The shield itself is made of 0.025 Mumetal and the idea was to evaluate, first of all, what kind of shielding factor we could get, by using a single shield to shield more than one component at a time. In other words, going to individual shields, this would actually turn out, in our case, to be bulkier than one large shield, because there were many of these reed switches. So, the first question was: How good is the shielding factor? The measured shielding factor was found to be 200.

The next problem was one that I don't think has been aired here. The problem is the environmental stability of the shield. Not knowing how environmentally stable they are, we ran some tests that I think quite exceed anything that you'll ever see for flight hardware. There is one little catch in these tests that we did run, and that is the shield itself is mounted to the environmental test fixture by a silastic compound, which is a silicon rubber. So, you do not have a hard mounting to the test fixture; however, it is to your advantage (if you are going to use shielding) not to make a rigid mounting, unless you have to, because point loading of the Mumetal doesn't do it any good. If you can keep from stressing the Mumetal, you will keep from destroying its annealed characteristics. I guess that is true of not just Mumetal, but all soft irons.



HEAVY LINES DENOTE WELDED EDGES
DIMENSIONS IN INCHES

Fig. 1. Magnetic shield configuration

The first environmental test was a shock test of 1500 g with a pulse duration of 1.25 msec. Each axis of the box was subjected to two such shocks. The residual field was measured after each shock test and remained below 1 gamma measured at 2 ft.

Now, we didn't try to make measurements in much closer than 2 ft; because if we can't see it at 2 ft, we certainly won't see it farther out.

Secondly, the box was subjected to a sine wave vibration of 40 g rms from 50 to 2000 cps in a nominal 150-gauss field. Now, we got the 150-gauss field by shopping around for the worst shaker on the Lab.

Again, the residual field was measured between shake tests and between each axis of shake, and it remained below 1 gamma at 2 ft. At the end of these tests, just to give it one final shock to see if we could louse it up another way, the box was thermally shocked by heating it to 90° C and then pouring liquid nitrogen on it, and that didn't do anything either. The residual field was again less than 1 gamma at 2 ft.

Now, actually, I think I was as surprised as anybody that the thing wouldn't perm up, but we just couldn't perm this box up to the extent that would be measurable with the resolution of 1 gamma at 2 ft. If one looks a little closer and goes in where you can see a field, at 1 ft it is something like 3 gamma. By pounding the shield on the floor, and what have you, we could change the direction of the field very slightly but nothing very dramatic. We took some similar pieces of Mumetal that were not annealed and played the same game with them, and found that indeed, they would perm up to some measurable amount at 2 ft – several gamma at 2 ft. Plus, they were not very stable in their field direction. If you pounded it on the floor, it would change direction.

In the tests that we did run, obviously the lid is essentially a drum head, so it certainly went through some vibrational strains and, apparently, it withstood the environmental tests without any change in properties.

I believe that just about concludes what I have to say on shielding, except to say the box was made using the heliarcing technique, which seems to work quite nicely in obtaining a reasonable shielding factor.

OPEN DISCUSSION

VOICE: Why did you use 1500 g?

MR. NORRIS: If we used 700, someone would say, what happens when you go to 800?

VOICE: Then you don't get that in a spacecraft?

MR. NORRIS: No, you don't.

VOICE: Did you notice any change in the main characteristics of the reed itself?

MR. NORRIS: Are you talking about the reed switch?

VOICE: Yes.

MR. NORRIS: We didn't have the reed switches in the shield when we ran the test.

I doubt seriously if they would have survived that pounding treatment. We were primarily interested in the stability of the shield itself. The reeds have been qualified for normal environmental-type testing, but not this ridiculous stuff.

N66-11295

DEPERMING OF OGO

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I would like to very briefly describe the magnetic testing that we performed on the OGO-I observatory last April at Malibu. Mr. Iufer showed some of the pictures of the test site earlier, so I won't bother to repeat it.

First, in performing the magnetics test on OGO (simply because of the size of the spacecraft, namely, approximately 50 feet when deployed) it is: (1) impossible to do the complete testing in a coil facility; and (2) it is practically impossible to do a complete field mapping with everything on at the same time. So we divided the tests into two phases.

In the first phase, we performed a DC magnetic test, nonoperating, on the entire observatory, doing it piecemeal; that is, the most significant tests were those that were performed on the main body of the observatory, and then we independently measured each of the appendages. We found that the booms and the boom experiments had fields low enough so they could be completely neglected. The tests on our solar panels showed that fully-illuminated they were approximately 0.25 gamma or less at the position of the magnetometer and, therefore, fell within the accuracy of our other measurements; and so could essentially be neglected. Therefore, we were left with just the measurements to be made on the main body of the observatory.

The OGO body is essentially a rectangular box, approximately 6 by 2-1/2 by 2-1/2 ft. The mapping tests were done by taking the observatory at the Malibu test site and mounting it in the spacecraft roll rings, which allowed us to rotate it to any angle about its horizontal axis. Mount this on a nonmagnetic turntable, rotate it, and obtain magnetic field measurements during the rotation from nine single-axis flux-gate magnetometers located actually in three triaxial sets at 6, 12, and 20 ft from the center of the observatory.

These were sampled sequentially through a scanner magnetometer system, which I will not attempt to describe in detail at this time, and the data were recorded on punched paper tape so that it could be fed directly into a computer. In addition, during the test we added a fourth triaxial magnetometer at the 24-ft distance, which is approximately the location of the magnetometer experiment.

In performing the mapping then, the observatory is brought in, mounted on the turntable, rotated through five complete revolutions, rolled through 90 deg on

the roll rings, rotated again, and then removed from the facility so that we can again check the zeros, compensation currents, etc., on the individual magnetometer probes. The temperatures of each of the probes, the compensation currents for each probe, plus similar data for the monitor magnetometer (which is located part-way up the hill to keep track of variations in Earth's field), and also the position indication for the turntable, are also recorded on the punched paper tape.

During this test we also have the capability of looking at the output of any of the triaxial magnetometer systems on Sanborn recordings. The initial tests that were run, the DC field mapping, indicated a field of approximately 2.4 gamma at the position of the magnetometer. At this time NASA made the decision that we should deperm the OGO. There had been no plan or thought given to this at the time, and our design constraint – slightly different than the one discussed a little bit earlier this afternoon – was depermed within 1-1/2 wk.

So the approach to this, of course, (if you are going to do it in this length of time) is: what do you have available. We combed the Los Angeles area and we were able to find slightly over 1500 lb of No. 9 wire available. We had a power supply and a controller that we used for black box demagnetization. We were able to get a second supply that was loaned temporarily by the experimenter.

We were then left with the constraint of trying to see what is the best deperming job that we could do with 1500 lb of wire, two 36-v, 30-amp power supplies, and about 1 wk to get ready.

The design that we chose for the deperming coil was an eight-sided coil. We required a minimum of 60 in. inside clearance, to be able to rotate the OGO. We had about 65 to 66 in. clearance between the turntable and the wall of the building, and we also had a constraint that we had about 8-ft doors in the building, so any coil that would work would have to be assembled inside the building at Malibu.

We decided on using seven turns. It would be a horizontal coil. The coil would be mounted horizontally, and also include the provision to buck out the vertical component of the Earth's magnetic field. The perming operation was then performed by rotating the observatory in the coil during the deperming operation.

We started with a peak field of approximately 30 gauss at the center of the coil; the maximum we could have gotten out of the set of power supplies that we had was approximately 36 gauss. We had paralleled the two supplies, and did not get 100% match between them. They did perform quite well, however.

We did the final assembly of the coil itself inside the Phase I Magnetic Test Building, building a steel strut within the coil so that it could be rotated, mounting it on eight wheels, and putting a point of rotation into the floor of the building so that we could rotate it. The only method of rotation that we came up with, which was within the design restraint, was to borrow a garden tractor from the fellow who was designing the coils and push it around with that. This did prove highly successful.

We fabricated the coils in 4 days, wound them in 3 days, and managed to run our test within the time period that was allotted.

Needless to say, there are a lot of things that we should have looked at in the design of this coil system for the protection of the observatory, but we just didn't have the time to do it until after the fact. The OGO observatory main body is mounted within the deperming coils. We removed the wheels, which are magnetic and mounted it on wooden platforms. The observatory is mounted in roll rings, and is brought in and out of the building.

The cradle is mounted to a nonmagnetic turntable, which is driven by something similar to a grandfather's clock (just wind it up by raising some weights and allow them to fall), and through a mechanical drive we rotate the observatory at approximately 1 rpm.

During the deperming operation, the entire cycle for deperm took approximately 4 min. We repeated it with the observatory at each 45 deg orientation. We rotated during the deperming, and then rolled 45 deg and rotated again. We went through a total of about some 15 or 16 deperming operations and found that, after the first two, there was a negligible change in the observatory field.

The results of the tests showed a reduction in the magnetic field. Again, at the position of the EP-6 magnetometer, it was approximately 1 gamma. All the accuracy in all the measurements here is approximately ±0.3 or ±0.4 gamma.

At the end of this particular operation, we then did (what we call) our Phase II magnetic testing, which is the mapping of the stray magnetic fields of the observatory. In these tests, the observatory is assembled, fully deployed, with all appendages on, except the two solar panels.

The rubidium vapor magnetometer was located in the center of the Fanselau-Braunbek coil system, so we could vary the magnetic field from essentially zero field up to Earth's field. In this way they were able to obtain data on the operation of the magnetometer at all field values.

The sequence here, then, consisted of running through some of the various operational modes of the observatory, starting out with worst case configurations, and then trying (if there were any problem areas that developed) to try to troubleshoot these and zero in on them. The stray magnetic fields, as detected by the DC magnetometer — in worst case configurations we get these by turning on everything we possibly could: all our gas jets, reaction wheels, telemetry systems, all experiments, then cutting all power to the observatory by the simple technique of pulling the in-flight jumper through which all power flowed. The maximum field variation that we saw was under 0.2 gamma at the position of the magnetometer. There were several stray AC field sources that were observed by either the VLF or the AC magnetic field experiments. Primarily, synchronization frequencies from power converters and, in one case of the AC magnetometer, an experiment-to-experimenter interference with the other experiments sharing the same long boom.

We were able to resolve this during the testing. About the only significant source left was the converter noise, and that only at the 2461 synchronization frequency, and its harmonics. So it turned out extremely well.

At the end of these tests, we went back and repeated our Phase I magnetics test, the field mapping, primarily to see what changes had occurred in the observatory field over this period of approximately 1 mo. The period during which the observatory had been handled in the Earth's field there had been mechanical work done around it similar to what would be done between this time and launch.

No special efforts were made during this time to take any precautions in working it or using any special tools that would not be used between that time and launch, in short, just through 1 mo of fairly normal handling and operation. The field obtained in the repeat of the Phase I test showed an increase of approximately 30%. However, the increase is still less than the accuracy of the measurements.

We feel that it is real because it was repetitive in each case. We would repeat the measurements from six to eight times. Whether it is a 10, 20, or 30% increase, we can't really tell.

The major sources of the remaining field can really be put down to four assemblies within the spacecraft. The command distribution unit, which contains 196 magnetic latching relays, and the problems associated with this are very well known. A horizon scanner that has four alnico bar magnets associated with it, one with each tracking head. We have the magnets matched and oriented in such a way that we have nearly a magnetic octopole, which cuts its contribution down to a couple tenths of a gamma at the magnetometer.

The third significant source is the two tape recorders, in this case the culprits are three relays and the permanent magnets associated with the erase head.

So, we feel that we have identified these sources. As far as further reduction in the field, as far as the command distribution unit is concerned, today there are better relays available magnetically than were available and qualified for use at the time we did the OGO design.

Some of those, for which the data was given earlier this morning, could possibly give us a factor of 5 or 10 decrease. I know there have been others that have been tested fairly extensively by STL on the Pioneer Program. This would be the major source of our potential increase without going to an all-solid-state command distribution unit, which today is feasible.

I think that, were the design changes to be made, the approach to be taken here would be more of getting rid of the relays wherever possible; and for the few that would remain, use those with much lower magnetic fields than those that were available at that particular time.

In addition, it has been possible to eliminate a majority of the minor sources of magnetic field. Miscellaneous hardware items, etc., which primarily for scheduling reasons, had to be used in some of the earlier assemblies. These have been replaced with titanium or brass hardware.

On the OGO-C tests, which are coming up in approximately 1 mo, we will be adding two more steps to the test program that was performed on OGO-A. In addition to the mapping of the field of the spacecraft as it is received and, also, deperming; before deperming, we will deliberately perm the observatory in a 25-gauss perm. The depermed field of OGO-C will be raised to 50 gauss, using new power supplies and controllers that are actually being provided by the Pioneer program.

So, for once, we are not able to furnish the equipment, but borrow it. In addition, we are making provisions for possible compensation of the DC magnetic field. It is not yet certain if this will be done, but right now the design specification is that if the permanent field after deperming is above 1 gamma at the magnetometer (and judging by OGO-A experience, it is going to be just about there), and if it appears to be a stable field, then we will compensate. The magnetic field compensation will be done by using a set of three alnico bar magnets mounted mutually perpendicular in an assembly mounted to one of the experiment mounting doors inside the observatory.

The test procedure here will consist of simulating the magnets by means of current loops (current coils) mounted outside the observatory, adjusting the current in them until the magnetic field measured at the position of the EP-6 magnetometer is nulled out as well as possible and, then, from this to actually determine what the field strength of the three individual bar magnets is to be. We will take the bar magnets, adjust their strength to the required value, mount them in the observatory, come back in, and recheck them and remap the field. Hopefully, this can be done in one or two attempts.

The final decision for doing it will be made by Mr. Parsons over there; so, I am glad that he has the problem. How well it will work if compensation is done, whether it will actually fly, remains to be seen.

Again, we will, after having done the compensation, go into the Phase II testing, and approximately 2 to 3 wk later we will come back and make a repeat measurement of the DC field. I just assume that, if there has not been any significant change in the compensated field, then the compensation assembly will remain in; but, if there has been a significant variation, and significant is going to be a tough term to define here considering that the types of fields we are talking about are very close to our measurement accuracy, then they may end up pulling it.

It will be interesting to see how it does work out and what sort of orbital data we get to see the effectiveness of the compensation.

OPEN DISCUSSION

MR. PARSONS: Just one brief word in my defense. The first time that I heard any proposal to fly the OGO with a 0.1 gamma sensitivity was about 2 yr before these tests took place, at that time I immediately said "Deperm it and take off all the iron you can." I was not an employee of NASA at the time, however, and my advice was not heeded.

The question of magnet compensation is one that we've tossed around before in the meeting yesterday. I don't believe Dr. Lindner was here at the time. As he carefully pointed out, if the magnet is used, it is to compensate hard perm. We hope to establish whether the perm is hard or not in this series of tests that will take place.

CHAIRMAN GAUGLER: On certain Navy magnetometers (when they are making very sensitive magnetometers) that end up with a certain amount they can't get

down, they deliberately do what they call "slugging" them. These have ferrite permanent magnets. They just solder them on the can. This is a technique.

MR. CANTOR: Obviously, this is well in the Earth's field. How do you get the accuracy out of the magnetometer then, as far as an absolute measurement down to the gamma level?

DR. LINDNER: Because the experiments are done in the Earth's field, it is necessary to compensate each of the individual probes. So, we have compensation coils wound around each of the flux-gate magnetometer probes. Before bringing in the observatory, at the start of the test, we will buck out the Earth's field around each probe. Then we continuously measure the compensation current for each of these probes.

METALLIC MATERIALS FOR NONMAGNETIC SPACECRAFT, PROPERTIES AND MEASUREMENT METHODS

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INTRODUCTION

The Navy has been interested in the class of metallic materials known as feebly, or nonmagnetic, alloys for many years. These materials are used widely in magnetic influence type mines as well as minesweepers and explosive ordnance disposal and recovery equipment. In 1951, a general survey of available nonmagnetic materials was made by the Naval Ordnance Laboratory (NOL) for the U. S. Naval Engineering Experiment Station, now Marine Engineering Laboratory*. In 1963, NOL investigated a group of nonmagnetic alloys for NASA (Goddard) at liquid nitrogen temperatures as well as at room temperature.

This paper describes the methods used for permeability measurements (a material property) and magnetic effects measurements (a component property). Materials that have been measured are classified according to their nonmagnetic properties. The results of NOL nonmagnetic investigations for NASA (Goddard) are also presented.

MEASUREMENT TECHNIQUES

PERMEABILITY MEASUREMENTS

The important magnetic properties in selecting a material are permeability and remanence. Materials with permeability values slightly greater than unity are called feebly, or nonmagnetic, materials. Because permeability is a function of the magnetizing force (H), the measurement of permeability should be made at the H field of interest. The circuit used for this measurement at NOL is shown in Fig. 1.

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*M. Pasnak and D. I. Gordon, "The Measurement of Magnetic Characteristics of 'Non-Magnetic' Material," NAVORD Report 2415
M. R. Gross, "Magnetic Characteristics of 'Non-Magnet' Metallic Materials Comparison of Properties in Strong and Weak Fields," E. E. S. Report 4E(2)66904, 4P(2)66918

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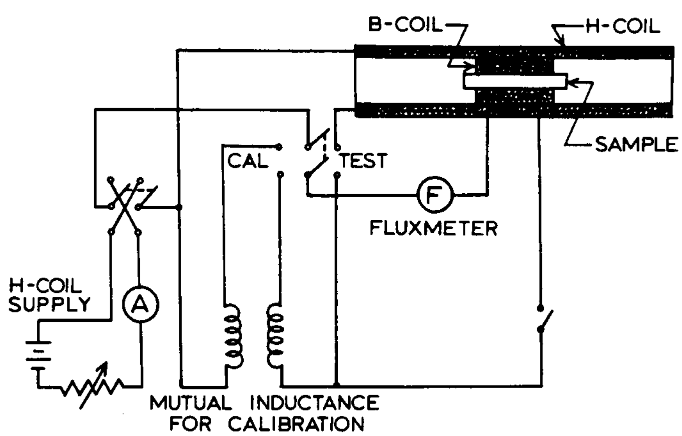

Fig. 1. Permeability measurement circuit

The fluxmeter is a Leeds and Northrup 2885E, which has a constant of approximately 2400 maxwells/cm of deflection. The H solenoid has a length of 53 cm and a diameter of 5.3 cm. A total of 6780 turns gives a constant of 162 oe/amp. The B pickup solenoid has a length of 15 cm, a diameter of 4 cm, and 4200 turns. Assuming a minimum readable deflection of 0.02 cm, and a sample of 1 in. diameter, the minimum measurable permeability at 0.5 oe is 1.004, ±0.004.

The measurement of permeability is made by (1) placing the sample in the B coil that has been centered in the H coil (both of which are aligned perpendicular to the Earth's field); (2) a deflection reading is taken without an applied H field as the sample is quickly withdrawn from the B coil; (3) the sample is then reversed and the identical procedure is repeated; (4) the above three steps are repeated for each value of applied field, finishing with a second test without an applied field. At each value of applied field, the field is reversed slowly several times to ensure that the sample is fully magnetized for that field.

From the above data the permeability, μ, can be found by the formula:

$$\mu = \frac{H+(K_2/NA)\,d}{H}$$

where

H = applied field

K_2 = fluxmeter constant in maxwells/cm

N = B coil turns

A = sample area in cm^2

d = fluxmeter deflection in cm

If the deflection readings obtained without an applied field have a value other than zero, the sample has a remanence. This remanence deflection reading should be subtracted from the deflection reading obtained at the desired applied field to find the deflection reading because of the induced magnetism.

Magnetic Effect Measurements

Magnetic effect measurements are used to check the magnetic properties of finished components. Both induced magnetism and residual magnetism are measured. The equipment is set up as in Fig. 2. The magnetometer is used to measure the change in the Earth's field because of the presence of the induced magnetism and/or residual magnetism of the sample.

The magnetometer is zeroed by electrically applying a field that cancels the Earth's field at the detector. Then any change in the Earth's field, because of the presence of the sample in the field, can be measured on a sensitive scale.

This method is used to check tools for explosive ordnance disposal applications as well as magnetic influence mine components, and is applicable to components for spacecraft.

An appropriately sensitive magnetometer system, such as this, would allow one to both evaluate a material before fabrication of a part and then, again, to check the finished component.

A magnetometer that could be used for this application is the ring-core magnetometer*. It has gamma sensitivity and can be inexpensively built.

Nonmagnetic Materials

Materials that have been measured by NOL, and found to generally have a permeability less than 1.004, ±0.004 and little if any remanence, are listed in Table 1. Among these is a NOL developed material, Nitinol; which will be discussed in detail in a following paper.

This material list can be used to select a material for a particular application. However, each lot of material should be evaluated before use, because properties may vary from lot to lot. Permeabilities are also subject to change because of cold-working or hardening processes. It is, therefore, desirable to evaluate each material in the condition that it will be used.

The above materials are the least magnetic of those measured at NOL. Although a complete survey has not been made, the list provides a useful selection of available nonmagnetic metallic materials.

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*W. A. Geyger, "The Ring-Core Magnetometer — A New Type of Second-Harmonic Flux-Gate Magnetometer," AIEE Trans., Pt. I (Communications and Electronics), Vol. 81, Mar 1962, pp. 65-73

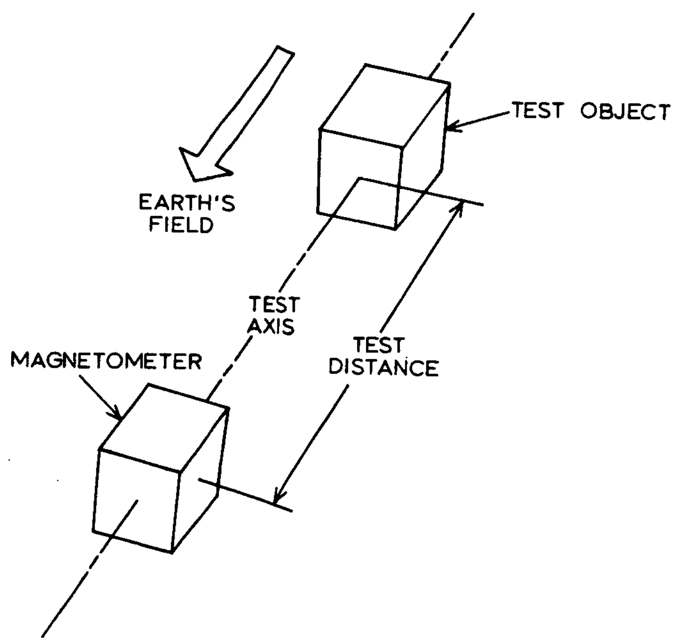

Fig. 2. Magnetic effects test geometry

Table 1. Nonmagnetic materials

Iron-base

- Wrought stainless steels: 305, 310, 316, 347, 25Cr-20Ni
- Precipitation hardening stainless steels
- Annealed austenitic Fe-Mn-Ni steels
- Annealed austenitic Fe-Mn-Cr and Fe-Mn (skinfree)

Nickel-base

- Nitinol (52 to 62 wt. % Ni, remainder Ti)
- "K" Monel
- "S" Monel
- Inconel

Cobalt-base

- Elgiloy
- Haynes alloy No. 25

Copper-base

- Precipitation hardening: 720 Alloy, Be-Cu
- Brasses, bronzes, cupronickel containing less than 0.2 wt % iron
- Annealed copper-based alloys containing between 0.2 and 6.0 wt % iron

Miscellaneous

- Aluminum alloys
- Magnesium alloys
- Titanium alloys*

*Not tested

Besides a careful selection of a nonmagnetic material with desired physical and mechanical properties, one must be on the alert for tramp and adventitious iron introduced during fabricating processes and by machining. This is especially true in the softer nonmagnetic alloy, such as aluminum, where iron chips can embed themselves during the machining process. It would, therefore, be desirable to test the magnetic characteristics of the fabricated part in its final form.

RESULTS OF NOL NONMAGNETIC INVESTIGATION FOR NASA

The measurements were made at both room temperature and at liquid nitrogen temperatures. Some of the specimens that would be sensitive to anneal and/or cold working were retested after annealing and/or cold working. The results are shown in Tables 2 and 3. It can be seen that Ni-O-Nel 825 was the only sample at room temperature to have a reading above 1.004, ±0.004, our lowest measureable permeability. K-500 Monel, Inconel Alloy X-750, and probably Ni-O-Nel 825 have a Curie temperature of about -100°C, which explains their high permeability values at -196°C. These same materials had sizable remanences at the low temperature. 720 alloy, 310 stainless steel, Haynes alloy No. 25, and the as received Elgiloy samples also had some remanence at room temperature. In general, this investigation was aimed at permeability measurements and remanences, unless large, were not measured.

CONCLUSIONS

It can be concluded that there are many available metallic materials that have permeabilities less than 1.004, ±0.004. Materials should be evaluated in the condition in which they will be used. Because properties may vary from lot to lot, each lot should also be evaluated.

An appropriately sensitive magnetometer system, such as that shown in Fig. 2, could be used to evaluate material and finished components. Such a system would allow one to both check a material before fabrication and again as a finished component.

Table 2. Feebly magnetic materials low field permeability

Alloy	As received condition	Additional processing	Magnetic permeability at 0.5 oe*	
			26°C	-196°C
356 aluminum	T6			
5086 aluminum	0, H32			
6061 aluminum	0, T6			
7075 aluminum	T6			
AZ31 magnesium	H24			
AZ92 magnesium	T6			
ZK60A magnesium	T5			
Yellow brass	Drawn	- - -		
		1000°F, 1 hr, A. Q.		
		1000°F, 1 hr, A. Q. +50% cold reduction		
2% Be copper	1/2 hard	- - -		
(Berylco No. 25)		600°F, 2 hr, A. Q.		
310 stainless steel	Annealed	- - -		1.011
		50% cold reduction		1.011
Ni-O-Nel 825	Annealed		1.008	1.61
A. Q. = air quench		All measurements are ±0.004		
*All valves are 1.004, ±0.004 unless otherwise stated				

Table 3. Feebly magnetic materials low field permeability

Alloy	As received condition	Additional processing	Magnetic permeability at 0.5 oe*	
			26°C	-192°C
720 alloy	Hot rolled	-----		1.099
		1050°F, 1 hr, A.Q., 33% cold reduction 700°F, 24 hr, A.Q.		
Haynes alloy No. 25	Solution heat treatment	-----		
		15% cold reduction		
		15% cold reduction 700°F 1 hr, A.Q.		
		20% cold reduction		
		20% cold reduction, 1100°F 2 hr, A.Q.		
K-500 Monel	Drawn and aged	-----		8.83
		1600°F 1 hr, W.Q.		16.5
		1600°F 1 hr, W.Q. 1000°F 8 hr, slow cool to 900°F, A.Q.		14.7
Inconel alloy	Drawn and aged	-----		1.40
		2100°F, 4 hr, A.Q.		1.073
		2100°F, 4 hr, A.Q., 30% cold reduction		1.036
		2100°F, 4 hr, A.Q., 30% cold reduction, 1300°F, 20 hr, A.Q.		1.84
Elgiloy	Hot rolled	-----		1.03
		2150°F, 1 hr, A.Q., 45% cold reduction		1.009
		2150°F, 1 hr, A.Q., 45% cold reduction, 950°F, 5 hr, A.Q.		

A.Q. = air quench W.Q. = water quench All measurements are ±0.004

*All values are 1.004 ±0.004 unless otherwise stated

OPEN DISCUSSION

MR. HAPPE: Ralph Happe, JPL. On your list of nonmagnetic materials, you have the PH stainless steels, which surprises me, because in their used condition they are martensitic. Are we talking about the same thing?

MR. LUNDSTEN: Now which ones were you talking about, please?

MR. HAPPE: In the 1951 measurements. In the stainless steels, you had the PH stainless steel as being nonmagnetic.

MR. LUNDSTEN: We did measure some that were nonmagnetic. I might imagine that some of these were annealed.

MR. HAPPE: Well, if they are in the annealed condition, that's true. Of course, in the normal used condition, precipitation hardening, they are almost 100% martensitic and, therefore, they should be magnetic and are magnetic as a matter of fact. There is, of course, A-286, an austenitic precipitation hardening steel, but that is not usually designated as a PH steel. It is considered in a separate category. Most PH steels, by terminology, are the martensitic ones and, therefore, would be nonmagnetic.

I might point out that the cold working effect can be related pretty well in some of the iron-base alloys, where you have strain transformations that can cause martensitic precipitation. That again ties in with martensite. But I don't see any need for testing cold-work copper-base alloys, where this kind of reaction is impossible.

MR. LUNDSTEN: Well, the trouble with some of the copper-based alloys is that they're relatively soft and could pick up trap iron, or something like this.

MR. PARSONS: I think I understood you to say, in your second paragraph, that you had a sample of 1-in. diameter, the permeability of which was something. Then, I believe that you said that this was the equivalent of so many gamma at so many inches. Could you reread those numbers again?

MR. LUNDSTEN: Well, if you start with a sample of 1-in. diameter that is 6 in. long, and you allow for a minimum readable deflection on the galvanometer of 0.02 cm, and you use the section of the formula that is $(K_2/NA)d$, this gives you the B measured, disallowing for any B because of H. Strictly the residual.

You plug in the 2400 for K_2 and 4300 for N, and 4.71 in.^{3} for the volume, and you measure it at 3 ft, and you come out with 0.008 gamma at 3 ft.

MR. WEINBERGER: Do you have any feel for the relationship of hysteresis loss as a function of permeability? You indicated that they had low remanence. I am working with materials that have higher permeability than 1.004; namely, 75% cold-reduced stainless steel, and I find high hysteresis loss. Do you have any information as to the hysteresis loss of these lower permeability materials?

MR. LUNDSTEN: Not specifically, but in general — if you don't have any permeability, then, of course, you don't have any hysteresis loss.

MR. WEINBERGER: Generally it seems that, as the permeability goes down (and I don't know where the breaking point is) the hysteresis loss goes up; or as we call it, the Steinmelz hysteretic coefficient. Is there a breaking point? Do you have any feel for what is happening?

MR. LUNDSTEN: I am not familiar with that.

VOICE: With some of the stainless, you can develop coercive forces of 200 or 300. Now, these are pretty good permanent magnets, and you would expect the permeability to approach one in a good permanent magnet and have a high hysteresis loss, and perhaps this is one of the things that you are trying to say: In a cold-work condition, you are seeing permanent magnetic effects.

VOICE: I would like to make a comment on copper. We have used electrolytic tough pitch copper, and we have had problems with magnetic contamination in it. This was eliminated by going to OFHC, mainly on the basis that it is handled better during fabrication. So, if copper has to be strictly nonmagnetic, I would recommend OFHC rather than any of the ordinary commercial grades, such as electrolytic tough pitch.

MR. IUFER: What is the alloy referred to by the letters OFHC, for those who don't know?

VOICE: Oxygen free copper.

MR. IUFER: I also have a question. It is noted in Fig. 1 that you are measuring the permeability of cylinders rather than rings. In the formula you showed for the calculation of permeability you did not introduce any demagnetization factor. How do you correct for the probability that mu is reduced because you have an open magnetic circuit?

MR. LUNDSTEN: Well, as mu gets small, the demagnetization factor falls out. We are not interested in measuring any permeability greater than 1.004. If you wanted to measure something with a permeability of more than two or three, you might run into problems.

CHAIRMAN GAUGLER: In other words, you get one minus mu, or one plus mu, something like that.

Didn't you originally measure the coercive force?

MR. LUNDSTEN: The coercive forces and the B_r were measured. The materials selected here were ones that had very low residuals. In most cases, if they had a low permeability they also had a low B_r. All this data is available in a couple of old reports.

VOICE: I would like to comment on this cold working of copper. We noticed cases where you take copper and copper alloys in the received condition, and you can actually have iron particles precipitated in these alloys so small that they are superparamagnetic. Yet, on working, these things can grow and elongate up to the region where they become permanent magnets.

Under those circumstances, you can see a tremendous disadvantage in cold-working copper alloys.

VOICE: Well, I can't give you numbers at this point; but, for instance, there is a process for making permanent magnets, using this kind of procedure where they purposely add iron to the copper alloy and then swage it to elongate the iron particles and come up with a pretty good permanent magnet.

VOICE: I am wondering if they get enough trap iron.

VOICE: Yes, we have seen this and it has given us trouble in alloys. So it is something that you should be aware of: it isn't always true that you don't get any cold work in the copper.

MR. PEIZER: This is a little far back for me: but my memory was, that on a number of materials the manufacturer would claim something like 1.01. I believe in your tests — I guess you made these tests — when they were actually measured, they might have been around 1.1.

MR. LUNDSTEN: Well, it went both ways. Some materials were not as good as claimed by manufacturers, and some of them were. It depended on the alloy, and it depended on the manufacturer. I didn't personally make the measurements in 1951.

MR. IUFER: Just a comment on our experience. I think this points out that you have to have 100% receiving inspection. We can pour out a gross of nonmagnetic stainless steel machine screws and find two or three that are quite magnetic and the rest are nonmagnetic. So, if they have lot control, they don't maintain individual lots when they package, or they may be short and someone throws in a handful of parts that were not as carefully controlled. So, the point of this is that the manufacturers' specifications for an alloy must be taken with a grain of salt. The only assurance is to check yourself or have a receiving inspection department make 100% receiving inspection for the magnetic property you want.

NONMAGNETIC HI-REL COMPONENTS

N66-11297

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What I am going to talk about today is actually a case study. I was the cognizant engineer of the large digital logic system that was going to be developed on the Mariner-Mars flight. This work was done something like 1 or 1-1/2 yr ago.

I got into the problem of nonmagnetic components because of the size of the system that I was working at. The size of the system we are talking about is a system of approximately 10,000 components, electronic components, that could be, depending on the spacecraft, anywhere from about one-fourth to one-fifth of the actual components used. So it did seem worthwhile investigating how we could reduce the magnetic properties of the components that we were using.

First of all, maybe I should talk a little about what I think, as a cognizant engineer, are some of the problems that you might encounter from magnetic fields generated by components (large or small) on a spacecraft, without getting in the obvious problems of a magnetometer.

We have two various types of systems on the spacecraft. We have analog systems and digital systems. Of course, in the analog system, the ground currents or potential differences between points that are fairly close to each other are extremely important and critical. In a digital system, usually these offsets (grounds not being at 0, 0.1, or 0.2 v) supposedly don't hurt you. This is one of the reasons we try to use the digital system when we can.

What happens, of course, is that in a digital system you have many repetitive circuits, many flip-flops, or many gates, so what you do is package these all in a repetitive manner. For instance, the way we were going to package these things in 1966 was to use welded cord wood modules. When you do this you end up with possibly your ground lines being anywhere from 5 to 10 times longer than they might be otherwise, because you try to interconnect points within a module, and you are trying to make the module, of course, as small as it can be; therefore you might end up with a ground line, instead of going from point to point with a ground line between — let's say, two flip-flops in a module — you may end up with 10 to 14 in. of ground line in a small module. So you end up with much more length than you had otherwise and, therefore, you end up with the fact that you can get to the point where it becomes noticeable — the actual ground currents induced by the magnetic field or any other pickup.

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So, there are two problems. One is that during systems tests, when you are trying to calibrate the system, you have these offsets. The other is that you are working with soft materials. Of course, these offsets change after you launch and a lot of your calibration can be useless. This is more critical with an analog system.

A couple of other things are: when you try to measure charged particles, either measure how many charged particles go through a certain size window in a Geiger tube or actually have an instrument where you try to curve your charged particle paths; or where you try to take TV pictures; anything can be affected by magnetic fields. All these instruments have problems from the magnetic field on a spacecraft. Most of them are more subtle. You hear very little about them except that they do exist, and mostly it is just a matter of whatever the field is, trying to calibrate the system with the existing field.

So, having some idea of what these problems are, I decided to try my best with this large system, and to try and get the magnetic field of this digital system down. I started by assuming that I would use welded cord wood modules and by talking to some manufacturers of components to try to get an idea of what standard lead material was available for our hi-rel JPL components.

First, I did an extensive analysis of what exact, specific components I would use on this system. I realized that I would really have to talk in specifics with these manufacturers. It is one thing to talk about a diode and it is another to talk about a specific diode, specific capacitor, etc.

I chose various components from our hi-rel lists, and from considerations of the circuitry; to get best margins in the circuitry, I started talking to manufacturers about these specific components. What leads they had were actually off-the-shelf leads. It was something that was actually sold on an upspecific procurement.

After doing this, I asked for samples of these leads from the various manufacturers and started to build up some sample modules. Then I built up a representative-sized module, using approximately 20 leads of each material. The leads were cut to about the length that they would be in the actual module.

One thing that is significant here, I don't know if many people thought of it, but when you are talking about a welded module you may gain a little bit in one respect when the lead material or lead length is much shorter than it would be in a soldered module, or if you used soldered components. You can afford to have a much shorter lead when you weld. So, therefore, you end up with maybe the total lead length of the component itself being in a 0.25-w resistor — it varies from one-quarter to one-half of the total length of the component itself. You end up with very short leads, which helps.

So, I actually built up a module of the lead length that would be used and measured various modules made of various materials. I built up three modules with just leads in them, and the lead material I used was Alloy 180, Alloy 42, and Dumet (gold-flash Dumet). I used Alloy 180 because most of the people that I had talked to who were familiar with the magnetic problem seemed to think that this was a good, promising material. That there were people using it (some of the NASA centers had been working with it quite a bit), and in talking to some of the manufacturers, I found out that this was not a standard lead material; but they said they could build parts using this material without any change. So Alloy 180 was used for this reason.

Alloy 42 was used because at that time I was under the impression that one of the manufacturers, the foremost manufacturer of resistors, was using this material on his resistors. Dumet was used because this was the common lead material for the capacitors and some of the diodes that we were using.

The measurements that were made were as follows. The three modules, after being permed with 1 kilogauss, at 3 in. measured 0 gamma for the Alloy 180, 40 gamma for the Alloy 42, and 50 gamma for the Dumet.

Now, at the same time I actually took 25 transistors, TO-18 can transistors that we would actually be using (off-the-shelf transistors), and with their leads cut as short as I could cut them, I measured a representative field of these. Now, I used 25 transistors arbitrarily to get some field that I could measure accurately.

In our typical module we would probably use something like eight transistors and possibly 60 of these leads that I have been talking about. The 25 TO-18 can transistors measured 300 gamma at 3 in. after being exposed in a 300-gauss field. I tried to have some measurements made with the nickel ribbon, and it turned out to be a very hard thing to do because we made a measurement one day and we tried to repeat it the next and we never could repeat out results. It seemed to be so soft that if you pinched it, it would change, or if you put it on a desk, dropped it one way, it would change; and if the temperature was different, you would get a different reading. It was so soft that I really couldn't give you any reading that I think is meaningful. The ribbon was 10 by 20-mil ribbon that I am talking about.

Now, after we did this, we built up three other modules. I actually tried to stimulate, say, two flip-flops in a module, tried to put them in the transistors and the various leads. Now on this I did two things. I put in the transistors and I tried to mix lead material by building up three modules. In all modules I used eight transistors and 14 in. of nickel ribbon, which I felt was representative of the module having a top and bottom welding matrix.

In module A, I used 60 Dumet leads. In module B, I used ⁄ Alloy 42 and 20 D. .et, and in module C, I used 40 Alloy 180 and 20 Dumet. Now, :se lead lengths were the same as discuss·d before. The field before any shake, ju. actually after fabrication of these modules at 3 in. was 42 gamma with the 60 Dumet leads, 19 gamma with the 40 Alloy 42 and the 20 Dumet, 68 gamma with the 40 Alloy 180 and 20 Dumet; which shows that either you have a wrong measurement or you are completely independent of what the lead material is, or you are dependent on orientation of leads.

On these modules we first tried to make sure that we oriented the components in the exact same manner, we tried to make sure that we handled the leads and components in the exact same manner, and we repeated these measurements. So what this reading before shake shows — if this were the only reading it would show — that the lead material is inconsequential as to the meaning of what the gamma reading would be under the module itself; because, using the nonmagnetic lead materials, we ended up with the highest magnetic fields.

After shaking in a 1 kilogauss field at 3 in., module A with the 60 Dumet leads was 152 gamma at 3 in.; module B, which was the 40 Alloy 42, went up to 60 gamma; and the least magnetic, supposedly, the Alloy 180 module went up to 271 gamma at 3 in. We degaussed these modules to try to start again. This is something, of course, that you wouldn't be able to do normally because of the possible induced currents in these components.

Then we measured the field again after a 1 kilogauss perm. We found that at 3 in. the 60 Dumet lead module was 546 gamma, the 40 Alloy 42 was 560 gamma, and the 40 Alloy 180 was 472 gamma. Now this shows that it was slightly less than the other modules, but not very significantly less. Certainly, it wasn't more, now. But we still didn't get the gain that we thought we might get by using the nonmagnetic leads.

After I made these measurements, I tried to make some calculation of what the total magnetic field of the data automation system (which I was working on) would be, based on the number of modules, how they would be oriented, etc., to see if (by not changing anything) I would actually pass the spacecraft specification or not, and I came up with the following conclusion: that I would probably have somewhere between 15 and 20 gamma at 5 ft in my total system, which would be out of specification.

So, I then decided that I was certainly warranted in looking into the problem further. I started talking to manufacturers about nonmagnetic lead materials.

Without going into the names of the manufacturers, the capacitor problem: the capacitors were being made with a certain lead material that they recommended as being a weldable lead material, which turned out to be fairly magnetic. At the present time they would not recommend changing that lead material and still guarantee a hi-rel part, because they were glass capacitors and they were talking about a metal-glass seal that they had to pass.

In talking to a resistor manufacturer, he was more than willing to go to Alloy 180 as a lead material, except that he would have to change the wattage rating of the resistor by one-half, a 0.5 w resistor, because of the lower conductivity of Alloy 180 with respect to, say, copper or Dumet leads.

We talked to one of the manufacturers of the transistor we thought we would be using, and it was learned that there was what they called some nonmagnetic transistor metal cases being made for a specific company, a vendor of theirs. We asked for samples of this case and they sent down a few. We actually measured these and compared them to the other TO-18s. It was found that by doing nothing but changing the case, by retaining the Kovar header and the Kovar leads, the magnetic field of the nonmagnetic transistors was approximately one-twelfth of the standard off-the-shelf transistor.

From consulting people here at the lab, who worked more with the components themselves, it was felt that none of the real processes had been changed to the point where we couldn't count on this transistor to be a hi-rel transistor. So, it was seen that this would be a tremendous gain in itself, if changing nothing else, to use this different case material (which turns out to be a stainless steel of some type). In asking the transistor manufacturer more about the problem of the header, how easy or how hard it would be to change the header material to get rid of Kovar, he felt that it was actually fairly impossible to do because I was talking about schedules of a couple of months and he was talking about the fact that it took them about 4 yr to come up with something like Kovar and the reliable transistor that they had.

So we decided to take some time and find some manufacturers of headers, because it was realized that transistor manufacturers didn't actually make the headers, they bought them. We tried to find out what the problems were with making a nonmagnetic header. We found a company locally that produced transistor headers, and not only did they produce transistor headers but they stated that they had a material that was nonmagnetic, had the same thermal properties as Kovar, better electrical conductivity than Kovar, and could be welded without any problem.

We actually gave this company specifications to produce headers. We asked for a sample of the material, and they sent us some material and leads. We made a quick analysis here at the lab to see how easy or hard it would be to weld this material, and it was decided at that time it wasn't very straightforward to weld this material but that it could be welded. So, on that basis of a real quick analysis, we went ahead and ordered 200 headers from this company to specifications that would meet the specific transistor, TO-18, can.

We got these in and shipped them to the transistor manufacturer on a procurement to send us back 100 hi-rel transistors, completely nonmagnetic-metal transistors. After 1 or 2 mo and various management shifts in the transistor manufacturing company, it was found that they were having a problem with the welding, welding their nonmagnetic case to this nonmagnetic header. We felt that the best way to handle that problem was to tie the header manufacturer with the transistor manufacturer and not have us the middleman; then let them see if they could solve the problem.

This was on for another 1 or 2 mo where the header manufacturer actually went up and visited the transistor manufacturer, gave them weld schedules, etc.; they discussed electrodes that should be used; and everything looked like it would be quite rosy. Then, about 1 mo later, after getting the new materials and trying it for another month or so — by the way, the transistor manufacturer did this in sort of the R & D section of the company — it was found that they still could not successfully pass the hermetic test. They could certainly weld the can to the header, but they could not pass the hermeticity tests imposed by the hi-rel specification.

So, within the last 1-1/2 mo now, what has been done is that several dozen of these nonmagnetic cases have been brought down to the lab at JPL and we have headers, and what we are trying to do now is to develop a schedule ourselves — first of all, try to determine what the real problem is in welding these two together.

Now, what we have been able to decide so far is that the header material is gold-plated. Now, I guess this is common practice, especially with transistor headers, to gold-plate, one for the gold bonding and the other is just for oxidation protection. What we have been able to decide is that the main problem right now is that the actual basic header material melts 200° F below the gold. So, when you try to weld, you essentially melt your basic material before you change your gold and, therefore, you don't get a good weld.

What we intend to do is to procure some unplated header material and we intend to start seeing if this can be welded to this nonmagnetic case. There are

many alternatives. One is dropping this material altogether and looking for other nonmagnetic material, because we now have a feeling for what can and cannot be done. Another possibility is trying to develop a feeling that we have a good header material and trying to develop another case material. We can't use the same material for the case, because this material cannot be welded to itself. So, right now, we have many alternatives, and there is no doubt that we could now use a transistor, we could use the standard Kovar transistor with the nonmagnetic case and, probably (where one takes a count of the total transistors in the system), we could halve the total magnetic field of the system.

The main thing that bothers me about this is that you can talk about the nonmagnetic materials, you can talk about the theoretical properties they have, you can talk about the weldability, and you can talk about what the manufacturer will give you; but, until you actually try to do something, try to ask the manufacturer definitely for a specific product at a specific time and realize that any of the work you are doing in a research lab, depends on — when the final chips are down, when you need hundreds of transistors — the transistor manufacturer who has to give you these and, therefore, he is the guy that has to make these things, he is the guy who has to know how to weld them, and he is the guy that has to know how to procure them and know the problems of them.

You end up with a realization that more and more of the work that you do should definitely tie very closely with the manufacturer, instead of working too long by yourself in some advanced development lab. We found out that this nonmagnetic header material could be welded successfully without any gold plating, and we started becoming very proud of ourselves because we developed a transistor that could pass the hermeticity tests; but we may go to the manufacturer and find out that the transistor won't even work without gold on that header.

So, this whole problem has to be tied very closely with the component manufacturer.

Now it is my understanding that there is a national committee to determine standard lead materials for components in the United States. It is also my understanding, from reading some of the minutes, that the magnetic or nonmagnetic properties of these materials either aren't entering into discussions or that they are secondary or very low on the priority lists, as far as developing standard lead materials are concerned. I don't understand this, because it seems to me that since there is a lot of work going on right now to actually try to standardize lead

materials because of all the problems, not only in the material itself, but the uniformity of material. The uniformity of material and the uniformity of diameter over the lead length for welding properties, etc. At the same time, there should be adequate representation of what the magnetic properties of these materials are, and also adequate representation of not only what leads you would like to use but can you actually get them.

For instance, I have heard here in the last hour some mention of oxygen-free copper. From all that I can find out, it seems as if it would be an excellent lead material to use, both from welding the material and also because you have a nonmagnetic material. But, about a year ago, when I talked to some manufacturers, I came to the conclusion that there was oxygen-free copper and there was oxygen-free copper, and there was the type that could be produced in a research lab and there was the type that the manufacturer was going to get for his unit, and they weren't necessarily the same. He didn't feel that he could guarantee the purity of this copper.

So we have to tie our theory in with actual practice when we talk about these materials.

What I think ought to be done on the whole nonmagnetic component problem is that there should be some central organization in the United States that actually keeps abreast of what the component manufacturers do put out as their standard leads.

Now I might digress a little bit in this. I have actually spent weeks trying to find out what the standard lead material is from a manufacturer. In other words, I would come to maybe five different conclusions in those 2 wk as to what a standard lead material was by talking to various people in his company, to people in our components area, to people who are supposed to know, and it always came to the same point where you didn't know what his standard lead material was until you got it in to make a chemical analysis of what his lead material was.

There should be some organization that worries about this. There should be some organization that publishes a list, if possible, of what a standard lead material is; not necessarily that it is just 40% nickel, 60% silver, but what it is also called. Is it called Alloy 42? What is Alloy 42? Does one manufacturer call Dumet one thing and another manufacturer call Dumet something else? I found this to be the case, when they talk about silver leads that have no silver, etc.

So, we could have a list published of current manufacturers' standard lead materials, which you would actually purchase if you went out and bought a transistor without specifying what the lead material should be, what you would actually purchase if you bought diodes, capacitors, and resistors from various manufacturers. Also, another list that would show what is available on request, and what penalties you

would have to pay — for instance wattage degradation, time (instead of it being delivered in 6 wk maybe taking 6 mo), and the cost of the item itself.

Without these, without this organization of all this material, I think that we are going to find that if we are really serious about worrying about these problems, as far as components are concerned, there are going to be a lot of engineers in the United States doing the same work over and over. They will be wasting a lot of time that need not be wasted, and they will have a lot of confusion.

OPEN DISCUSSION

MR. IUFER: I noted with some alarm that you were exposing these parts to 1 kilogauss fields. The comment is: that this is fine for study, but no one should do this on any material that they plan to actually fly, because of the problem of reducing the perm later, and the stressing you might apply to the junctions and what not in removing this perm.

Pioneer and future work at Ames is very interested in this problem of metal-to-glass seals. We have not beat the Kovar problem. If anyone has any information on this, or some guidance, I personally would be very interested in it. We use Kovar and Dumet as it is currently manufactured, and I might point out that we were able to get low field levels on Pioneer using this material. As far as resistors are concerned, on Pioneer we do not permit any magnetic field allowance for resistors. They have to be clean.

MR. FRIEDMAN: How do you do that?

MR. IUFER: We use oxygen-free copper.

MR. FRIEDMAN: You do?

MR. IUFER: Allen Bradley.

MR. FRIEDMAN: Do you weld these?

MR. IUFER: Yes. There is no nickel ribbon used on Pioneer. It is all Alloy 180 or copper. We have had no difficulty with the heat conductivity of Alloy 180. Alloy 180 is manufactured by Driver Harris and it is an alloy of 23% nickel and 77% copper, and the copper content has met our heat transfer requirements. There is an Ames QC specification on lead materials. I believe that it is ARC 301, which specifies by name, alloy, and purity, the materials that are approved for Pioneer. This includes Kovar, Dumet, and Alloy 180.

Anyone who would be interested in having a copy of this specification, write Ames Research Center, attention E. J. Iufer, and we could forward one to you. This specification is passed on to vendors who make the lead materials, or procure the lead materials, for the parts. We find that their process specifications may not clean an alloy purity and there can be quite wide variations in the product that they finally deliver. We have found that it requires more skill to use Alloy 180 in welding than nickel. We find that the pull test, for example, may be degraded by a factor of four; however, if you spend time with the welding machine operator, and execute pull tests right in front of him after he has welded the samples, you can soon establish a weld schedule that does produce reliable results.

We were alert to this problem. It seems that on the first IMP, one of the scientific experiments was found to be intolerably magnetic. This was traced to the nickel ribbon used in the cord wood modular construction, and it was necessary for this experimenter to find a new vendor to build up new modules. I think that they were put on the old motherboards, and the experiment with Alloy 180 ribbon did meet the specification.

We have measured, I should imagine, something over 2000 modules and the difference between nickel ribbon and Alloy 180 is at least an order of magnitude.

MR. CANTOR: We are the group that has first pushed Alloy 180 for all the IMP welding, but we have problems. It does require a very narrow weld schedule setting, and so it is one of calibrating your material and keeping it pretty tight. However, we don't like copper, whether welded to nickel, whether to Alloy 180, whether to itself; we do not consider copper a weldable material.

MR. IUFER: As you can see, there is some difference in experience of centers. For a while there was an East Coast and a West Coast gamma too.

We recognize that Goddard also, if I understand correctly, does not allow any tinplate or solder coat on wires that are welded, whereas we have several contractors who are getting by with this very well. You cannot tell by subsequent testing, other than visual, that the materials have been solder-coated. So I think it depends on possibly previously bad experience.

You try a process that doesn't work and you stay clear of it and use safer processes perhaps.

MR. GOLDSTEIN: Goldstein from Texas Instruments. Something you might want to check into. About a year ago, our semiconductor components division developed a nonmagnetic version of the micromesa package; I can't tell you exactly

what is in it, but having run checks on it I have yet to see one that looks like more than 0.05 gamma at 3 in. Generally, there is no measurable field at all.

MR. FRIEDMAN: This is a metal transistor?

MR. GOLDSTEIN: No, basically the die inside is a ceramic carrier. I believe that it will accept just about any passivated die, within size limitations, that will fit inside that package.

MR. FRIEDMAN: Is the package itself metal? What is the seal?

MR. GOLDSTEIN: It is a hermetic seal. It is a metal-to-ceramic seal; it has been checked and it is an effective seal.

VOICE: You told us a story that had me on the edge of my chair with suspense about this nonmagnetic header material, and then you forgot to give us the punch line. What was the punch line?

MR. FRIEDMAN: Well, I don't think that I would like to mention the name of the material, it is a patented material, and I don't know that it's proper to name the material itself.

VOICE: Well, if it's patented, I think you could. In fact, I think you could give us the patent number.

MR. FRIEDMAN: Well, I could talk to you later. I would rather not say.

MR. WILER: I have noticed that there is one important type of component that has not been mentioned, and that is the magnetic component itself, transformers, inductors, that sort of thing. Do you depend on shielding to prevent any effect, or what is your consideration there?

MR. FRIEDMAN: The reason I didn't mention this myself is that, in my particular system, perhaps 1/2 to 1% of the components used would be transformers. So, I have enough problems of my own worrying about other components; that is the reason why we would just buy standard transformers. The transformers that we do use are working in the milliwatt region, they are very tiny high frequency transformers. So I would suspect that it wouldn't present much of a problem, compared to the rest of the system.

MR. IUFER: We have a little information on that. We have put the black spot on powdered iron cores. These are bad actors. Permalloy cores are used by and large and, to reduce the magnetic characteristics of these, you need an

extremely efficient magnetic circuit. What's good for the magnetic moment of the specimen is also good for its internal operation. In some cases we have had to go to somewhat elaborate winding techniques, so that the effective turn pitch is constant throughout the annular core. In fact, the experiment that uses by far the most toroids in it is the Stanford radio experiment. Their magnetic moment was the least of all experiments. It has been Kovar whenever we have had trouble.

MR. NEAL: Neal, Motorola. We delivered a few thousand welded cord wood modules for the Mariner Mars program. Our experience has shown that a major problem is that there is no standard lead material, even for the hi-rel components. It is essentially what is available. Even where a relatively generic name is given for the lead material, the working characteristics or the fabrication techniques have a much larger effect on both the magnetic properties and on the welding properties of the material than the chemical composition.

So, we found that given tight-lot control on magnetics or weldable components, it is still necessary (wherever the weld schedule is tight) to run new tests on each new lot and that it will shift a tight schedule fairly sharply.

MR. FRIEDMAN: I didn't mention this, but I should. We found out, very quickly, after talking to manufacturers, that whatever lead materials we decided to use that we would definitely have to rewrite any of our component specifications. We would actually have to call out the lead name, the diameter uniformity, material uniformity, and percentage tolerances. I believe that if we would do this, we would get more cooperation from the component manufacturer.

MR. NEAL: But you are not getting a standard part, and you are to some extent compromising hi-rel characteristics in fabrication.

MR. FRIEDMAN: Right. The problem here is that in looking at the hi-rel components — the criteria of what a hi-rel component is — I think that the properties of a lead material itself (because that wasn't considered to be a part of the component) was always treated mistakenly as a low, low priority. It turns out, of course, that you can have a very reliable component, but if you have an unreliable weld from that component to another one, that component is not worth anything.

CHAIRMAN GAUGLER: I would like to make one comment on toroids. One experience I had with Mariner Mars — you know that you have this arcing problem in the critical pressure region, and the tendency was to order toroidal transformers, and sometimes the windings would be jumble-wound. So you might get as low as

150 v across adjacent turns, and they had arcing. You don't ordinarily get arcing with 150 v and with double hi-formex wire.

MAGNETIC CLASSIFICATION OF METALLIC MATERIALS FOR SPACECRAFT USE

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INTRODUCTION

The Mariner II spacecraft presented many magnetic control problems. The magnetic field intrinsic to the spacecraft was not only an order of magnitude greater than the ambient interplanetary field, but it also had a tendency to fluctuate and drift. Because of limited time, state of the art of low field magnetics, and reliability requirements imposed by the long mission lifetime, only cursory magnetic evaluations of some spacecraft hardware could be performed. Virtually no corrective action could be scheduled. A brief, rushed evaluation of the magnetic fields of the spacecraft spare assemblies indicated several magnetic problem areas that were eagerly pursued in the following 18 mo. One of these areas, to which this paper will be confined, is that of materials and stock metal products as related to the various magnetic environments to which they are subjected during fabrication, assembly, handling, and launch of a spacecraft.

For the various magnetic environments many questions were raised. During fabrication has the bolt or bracket been ground on a stone so that the surface may be imbedded with ferrous material from the last grind job? Was the part cold worked? What erosion rate of the steel drawing die may be tolerated before the special alloy wire becomes magnetic? On applications of stainless steel 440C, will nonmagnetic tools be used in assembly and installation? Can the spacecraft mechanics be prevented from checking a part with a magnet to determine if it is ferrous or not? Can the spacecraft transporter be made of aluminum or can it be depermed if made of steel? What is the magnetic field of the hook on the overhead crane and what is its significance? What is the field and its affect on the spacecraft from the gantry, the service tower, the launch vehicle? Can all the ferrous components of the spacecraft be expected to become magnetized in the earth's magnetic field during the launch dynamics? Where the ferrous components do become magnetized, what happens to the spacecraft field under the influence of the low interplanetary field? Can a realistic simulation of the launch magnetic environment be reproduced with a vibration

exciter and a steady magnetic field? What margin is exercised in magnetizing tests so that the test is not over conse vative? Finally, what usable material will remain stable under thes environments?

It would a pear sufficient to determine the maximum static magnetic field equivalent to the worst of these environments, translate this to standard magnetic parameters of permeability, remenance, coercivity, etc., and select materials meeting or exceeding these criteria. These parameters are defined for precise conditions of test as well as conditions of the material and, as such, relate to the microscopic properties of the material. They give little information about the magnetic stability of the material. A spacecraft carried magnetometer responds, in contrast, to the macroscopic or gross material magnetic field that is a function of the above parameters as well as mass distribution, geometry, history, and the environment of the material.

The instability in the gross material magnetic field of the spacecraft, as shown by Mariner II experience, is proportional to the total spacecraft magnetic field and is typically 10% or more. It is necessary, therefore, to use nonmagnetic materials in the fabrication and construction of a magnetometer carrying spacecraft if the desired low field environment for the magnetometer sensor is to be provided.

OBJECTIVES

For these reasons, a continuing investigation of magnetic classification of metallic materials for the purpose of seeking nonmagnetic materials for spacecraft use was begun in July 1962. A more recent phase of the materials work has sought to relate remnant fields with mass and geometry so that necessary ferrous materials on spacecraft may be configured to produce minimum magnetic field.

MAGNETIC EVALUATION CRITERIA

An estimate of the most severe hardware magnetic environment was 100 oersted steady field. Vibration testing to type approval levels on a 35 Gauss ambient magnetic field vibration exciter was a common environment of all flight hardware. Materials that permed during shake should be avoided. The 100 oersted perm tests are comparable to the shake test from a magnetic point of view. A safety factor of 10 was considered satisfactory for overtest. The 1000 oersted tests are very

conservative. Materials that did not perm up because of a 1000 oersted environment may truly be considered nonmagnetic for most spacecraft applications. From these environments, criteria were established defining three material catagories. For nonmagnetic or acceptable alloys, the field measured 12 in. from the sample must be less than or equal to 5 gamma after 1000 oersted exposure and less than or equal to 1 gamma after 100 oersted exposure. Borderline or marginal alloy magnetic fields measured at a point 12 in. from the sample were greater than 5 gamma after 1000 oersted exposure and greater than 1 gamma, but less than or equal to 5 gamma after 100 oersted exposure. Magnetic or unacceptable alloys were those, with fields measured at 12 in. from the sample, that exceeded 5 gamma after 100 oersted exposure. It will be noted that the distinction between the last two categories in practice is greater than that implied by the criteria. Although these criteria are based on sample size, it will be seen in the data that these are also natural divisions in the material magnetic fields.

All samples were in the form of cylindrical rods 2 in. long and 3/8 in. in diameter. This size was chosen for convenience, although slightly larger than the spacecraft components of interest.

PROCEDURE

A 13-step test procedure was formulated on the basis of the above environments and test criteria. Steps 1 through 9 of the test procedure classify the materials as one of the three above magnetic categories. Steps 10 through 13 provide both correlation with the 100 oersted magnetization test and information about the materials magnetic stability under realistic subassembly vibration tests. The test procedure was:

1. Magnetically map sample
2. Perm in 1000 oersted DC field
3. Magnetically map sample
4. Deperm in 600 oersted 60 cps field
5. Magnetically map sample
6. Perm in 100 oersted DC field
7. Magnetically map sample
8. Deperm in 600 oersted 60 cps field
9. Magnetically map sample

10. Shake in 35 oersted field to type approval vibration levels
11. Magnetically map sample
12. Deperm in 600 oersted 60 cps field
13. Magnetically map sample.

The magnetometer facility on the JPL Mesa was the location of all measurements. The single axis Fanselau coil system was used to produce the nearly zero field for the sample. A single axis saturable core magnetometer aligned with the center of the coil system working volume and at 1 ft distance from its center was used as the sensor. A turntable located with axis vertical and centered in the working volume of the coil system supported a wooden "V" block in which the cylindrical sample was placed. The output of the single axis magnetometer electronics was connected to the Y-axis of an X-Y recorder. The X-axis was driven by a helipot connected to the turntable by a wooden dowel rod.

Deperming and perming of the samples was carried out in the same solenoid. A DC power supply produced the current for the 5000 turn solenoid when used as a perming device, while 60 cps 115 v AC controlled by a Variac was the excitation for deperming in the same solenoid. DC fields of 100 and 1000 oersted were produced at the center of the solenoid for the perming tests. Approximately 600 oersted peak field at 60 cps was the deperming field.

The materials study has been historically in three phases. The initial materials evaluation and the stock fastener criteria make up Phase I. Phase II includes the nonmagnetic steel alloy postal survey and results. Phase III is the geometry and mass study mentioned previously.

PHASE I STUDIES

With the launch of Mariner II, a shift of some 40 gamma in the magnetic field at the nagnetometer sensor prompted the Phase I materials evaluations. The materials were, because of the urgency of the initial study, those available from Stores at JPL. Included were 43 ferrous and nonferrous metals and nine filler impregnated resins.

Stock Fasteners

As a result of the Phase I materials evaluation, criteria were established for stock fasteners. Machine screws, bolts, nuts, washers, rivets, and threaded inserts had produced significant distributed magnetic contributions to the total Mariner II magnetic field. The need for nonmagnetic stock fasteners was self-evident. High strength-to-weight requirements for stock fasteners on the Mariner Mars project reduced the number of candidate metals to three: titanium, and A286 and 303 stainless steels. For spacecraft purposes titanium and A286 are non-magnetic and mechanically preferable because the magnetic properties of 303 stainless steel depend on its history. The stock fastener magnetic criteria were set as 2 gamma/lb weight measured at 6 in. from the center of mass, with the stability requirements that no field change be observed at 6 in. after 100 oersted exposure. Thus, the magnetically marginal 303 stainless steel was virtually eliminated and an additional quality assurance check was available to prevent the accidental inclusion of a lower strength steel fastener in the spacecraft fastener stock bin.

PHASE II STUDIES

Realizing that the number of materials available for the Phase I materials evaluation was necessarily limited and that metals manufacturers are engaged in a continuous metals development program, it was decided to survey these metals manufacturers for new or unpublicized nonmagnetic high strength steel alloys applicable to spacecraft use. During September 1962, a form letter was mailed to approximately 1100 companies that represent the bulk of the free world ferrous metals business. This letter solicited each company's suggestions for new high-strength nonmagnetic steel alloys they produced that were usable in the U. S. space program. To this inquiry over 850 replies were received suggesting everything from pyrolitic graphite to powdered iron oxide as used in recording tapes.

From the list of suggested alloys 95 were chosen for additional study. Although these 95 alloys are not necessarily new, lack of information on magnetic properties warranted their evaluation and classification. The materials evaluated in Phase II did not duplicate those evaluated in Phase I except in a few instances where questionable data required reevaluation of the materials from Phase I.

PHASE III STUDIES

One of the elementary concepts of magnetostatics implies that a ferrous alloy will make a good bar magnetic if one dimension greatly exceeds the other dimensions. The converse is usually ignored and has provoked no discernable interest. Also elementary to magnetostatics is the proportionality of a magnet's field strength to volume or weight. Both concepts, of course, have definite limitations. Both are implied for fields greater than a few oersteds. Yet, one of the first thumb rules gleaned from the Mariner II experience was that, within a small limit, the magnetic field of a subassembly was proportional to its weight. The obvious question then was "Do shape and mass relations exist for the magnetic properties of a given alloy at fields under 1 oersted?" Eight mild ferromagnetic materials have been chosen for this study. This phase of the study is in preliminary stages now. No inferences can yet be made.

GENERAL RESULTS AND CONCLUSIONS

The experimental results of Phases I and II are given in Tables 1, 2, and 3. The alloys that, for spacecraft purposes by the proceeding criteria, are nonmagnetic are given in Table 1 and include a wide variety of both ferrous and nonferrous materials. Some materials where known by more than one name are listed under each common name to facilitate identification by the design engineer. As desired, the bulk of the materials suggested and evaluated have fallen in the nonmagnetic category.

Only a small number of materials has been grouped as borderline alloys as listed in Table 2. Although these alloys may be found under specific situations as nonmagnetic they require special investigation when used aboard a magnetometer-carrying spacecraft. This is particularly true of some of the 300 series stainless steels because of chemical composition tolerances and variations in magnetic properties with hardness or cold reduction.

The magnetic alloys listed in Table 3 require little or no comment. The magnetic fields retained after high field exposure obviates their hindrance to spacecraft magnetic cleanliness.

In conclusion several observations are appropriate.

1. Although this approach to nonmagnetic materials selection may be thought unconventional, a basis that is practical in terms of spacecraft magnetic fields has been established. It is simple, reproducible, and provides sufficient information. Similarly, the material grouping criteria are practical for contemporary spacecraft.

Table 1. Nonmagnetic alloys

Metal	Field (gamma) after	
	10^3 oersted	10^2 oersted
Titanium 75A	0	0
6A14V	0	0
RC55	0	0
RC130-A	0	0
RC130-AW	0	0
RC130-B	0	0
RS55	0	0
Stainless steel 310	0	0
347	1	0
19-9DL	5	0.5
Aluminum 2024-T4	0	0
6061-T6	0	0
7075-T6	0	0
K Monel	0	0
KR Monel	0	0
Molybdenum	0	0
Magnesium	0	0
Berylium copper	0	0
Aluminum bronze	0	0
Johnson bronze	0	0
Phosphur bronze	0	0

Table 1 (Cont'd)

Metal	Field (gamma) after	
	10^3 oersted	10^2 oersted
Stainless steel A286	0	0
Hastelloy D	1.2	1
CF-30 (111717-A)	0	0
CDC 730	0	0
Stainless steel 316, 316-L	0	0
CK-20 (572-BB)	0	0
Stainless steel 333	0	0
CDC 720	0	0
Cobalt tool steel	0	0
RA-600	0	0
Incoloy 800	0	0
Hardsteel Hd-32M	0	0
Chromel D	0	0
Copel	0	0
Alloy 843	0	0
Chromel A	0	0
Firth Brown NMCW	0	0
Firth Brown FNMC	1	0.8
OFHC copper	0	0
Alloy 53 (Coast Metals)	0	0
Hadfield 14% MN A-128-33	0.8	0
Kennametal K-601	0	0
Chromel AA	0	0
17-14 CuMo	0	0
Hella 8302 MN steel	0	0
22-4-9 SS	0	0
Monel K-500	0	0
Resista PH Hi MN Steel	0	0
Inconel X-750	0	0

Table 1 (Cont'd)

Metal	Field (gamma) after	
	10^3 oersted	10^2 oersted
Alloy 64 (Coast Metals)	0	0
Inconel alloy 718	0	0
Zinc	0	0
Inconel alloy 700	0	0
VX-9338	0	0
Inconel alloy 722	0	0
Nichrome 5	0	0
Haynes Star J alloy	0	0
Inconel alloy 901	0	0
Haynes alloy no. 31	0	0
Inconel 600	0.5	0.5
Inconel X750	0	0
Haynes R-41	0	0
Hastelloy alloy B	0	0
Haynes alloy no. 25	0	0
Haynes alloy no. 12	0	0
403 Monel	0	0
Hastelloy alloy C	0	0
Alloy no. 90	0	0
Haynes 98M2	0	0
Alloy no. 800	0	0
Alloy no. 30	0	0
Calo 74117	1	1
Pyro 74302	0	0
Alloy no. 45	0	0
Alloy no. 60	0	0
Alloy no. 180	0	0
Haynes Stellite alloy no. 21	0	0
Haynes Stellite alloy no. 19	0	0
Haynes Stellite alloy no. 3	0	0

Table 2. Borderline alloys

Metal	Field (gamma) after	
	10^3 oersted	10^2 oersted
Stainless steel 303	3	0
304	23	1.5
321	9	3
RA 330	2	2
S-28 (Coast Metals)	6	4
Ni-Resist D-2	4.5	3
Ni-Resist D-2B	2	2
Haynes alloy no. 3	2	1.5
Haynes alloy no. 6B	3	3
Haynes alloy no. 6	6	4
Haynes alloy no. 40	3.5	4

Table 3. Magnetic alloys

Metal	Field (gamma) after	
	10^3 oersted	10^2 oersted
Stainless steel 410	525	195
416	525	193
440	990	169
Monel	125	138
Nickel	1250	710
Iron Oilite	575	295
Steel 4130HT	610	282
4140	725	177
4340N	490	170
Cold rolled	285	113

Table 3 (Cont'd)

Metal	Field (gamma) after	
	10^3 oersted	10^2 oersted
Steel Drill rod	540	238
Graph-Mo	525	161
Nitralloy	400	151
Screw Rod	150	56
Starrett	545	358
Armco iron	77	33
Invar	-	67.5
Alloy 56 (Coast Metals)	21	20
Kennametal 3411	540	78
Kennametal 3109	645	45
Kennametal 3047	570	57
Kennametal K-151A	100	12
Kennametal K-162B	170	35
Kennametal K-6	380	8.5
Kentanium K165	90	6
Kentanium K138A	16	25
Kovar	225	14

2. The material lists, though current, are not complete. As new materials are made available they will be evaluated and added to these lists. The mass and geometry study also requires further emphasis.

3. While actual magnetic field reduction through use of the Table 1 nonmagnetic materials cannot be assessed at this stage, it is believed to yield the greatest reduction in spacecraft magnetic fields. Certainly, the use of nonmagnetic materials is the most effective means of spacecraft magnetic field control.

4. Although the list of nonmagnetic materials is finite, there appears to be an adequate selection for the spacecraft designer. With the nonmagnetic materials available, the preferred low spacecraft magnetic field environment can be expected.
5. A major effort to provide close consultation to the design engineers is needed during initial spacecraft design to ensure adequate consideration of the nonmagnetic requirements and suggested materials with other design requirements.

OPEN DISCUSSION

MR. PARSONS: Did I understand that the samples were all the same size; that they were 3 in. in diameter and 2 in. long, and this is a solid chunk of metal?

MR. BASTOW: No, they were 2 in. long and 3/8 in. in diameter.

MR. HEINDL: Joe Heindl, Space Technology Laboratory. I would like to make a comment about the A-286. In our experience we found that if it is cold worked after the solution heat treating, you will get a very high magnetic field, as compared to if it is cold worked before solution heat treating and aging. So, I think this is one that you would like to consider the condition of before you use it. It is a commonly used material in lock nuts and, also, captive nut plates, which are very widely used.

CHAIRMAN GAUGLER: I am not a metallurgist, but I know that if you get a stainless steel and it is nonmagnetic and you drill a hole in it or something, then you do get a magnetic moment.

MR. GOLDSTEIN: This might be a good point to comment. Several speakers have mentioned the problems that you get into when you machine some supposedly nonmagnetic material, and then you go and check it and it turns out hot. We have a requirement on drawings for nonmagnetic parts that they be machined only with carbide tools, and this is one way to get around it.

We also require that any grinding be done with a wheel that has never been used for any ferrous machining.

VOICE: This is not a cure, because a carbide tool is a magnetic material. It is a sintered carbide with a cobalt binder. If you imbed some of the cobalt material

in the part, you may have a magnetic part. So it is not a complete cure. There is a lot less chance of it happening, but it can happen.

VOICE: I would like to take some issue with the rather sweeping conclusion in the paper that there appears to be enough nonmagnetic materials to build a spacecraft. I don't want to delve into the whole thing but, for example, about 99% of all ball bearings are made of nonmagnetic materials that are not on the list.

CHAIRMAN GAUGLER: Well, we are going to give you one in a few minutes, in the next talk.

VOICE: There are problems in doing this.

MR. LUNDSTEN: We use the carbide tool to first clean up our specimen and then we take a small skim with a stellite tool bit, which doesn't bring in these problems.

THE UNIQUE NONMAGNETIC NITINOL ALLOYS

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INTRODUCTION

Recent advances in space and undersea technology have placed an ever increasing burden on the materials scientists to develop new materials with unique combinations of properties. A partial list of desirable properties might include the quality of being nonmagnetic; having low density; possessing high strength at both room and elevated temperatures; having resistance to oxidation, abrasion, and corrosion; along with such special properties as mechanical vibration damping over a wide range of stress and temperature. Obviously, no single existing material possesses all these desirable properties. However, in an effort to meet some of these demands, the metallurgists and crystallographers at the U. S. Naval Ordnance Laboratory, White Oak, Maryland have been conducting research on intermetallic compound base materials.

What is an intermetallic compound? By handbook* definition an intermetallic compound is "an intermediate phase of an alloy system, having a narrow range of homogeneity and relatively simple stoichiometric proportions, in which the nature of the atomic binding can vary from metallic to ionic."

To further describe an intermetallic compound, two hypothetical equilibrium diagrams that include representative types of compounds are given in Fig. 1. The equilibrium diagram (or constitution diagram) is one of the metallurgist's principal tools in studying phase equilibria existing in the solid state in a two or more component metal system. Observing the diagrams given, it can be seen that the hypothetical compounds A_XB_Y, C_LD_M, etc. are, as the definition indicates, intermediate phases in an alloy system having a relatively narrow range of homogeneity. In addition, it should be pointed out that a compound can exhibit congruent or incongruent melting (C_ND_O) and often a compound may have a melting point higher than either of the two component metals from which it is formed. This can be seen for compound C_LD_M in Fig. 1b.

*Metals Handbook, American Society for Metals, 8th ed., 1961

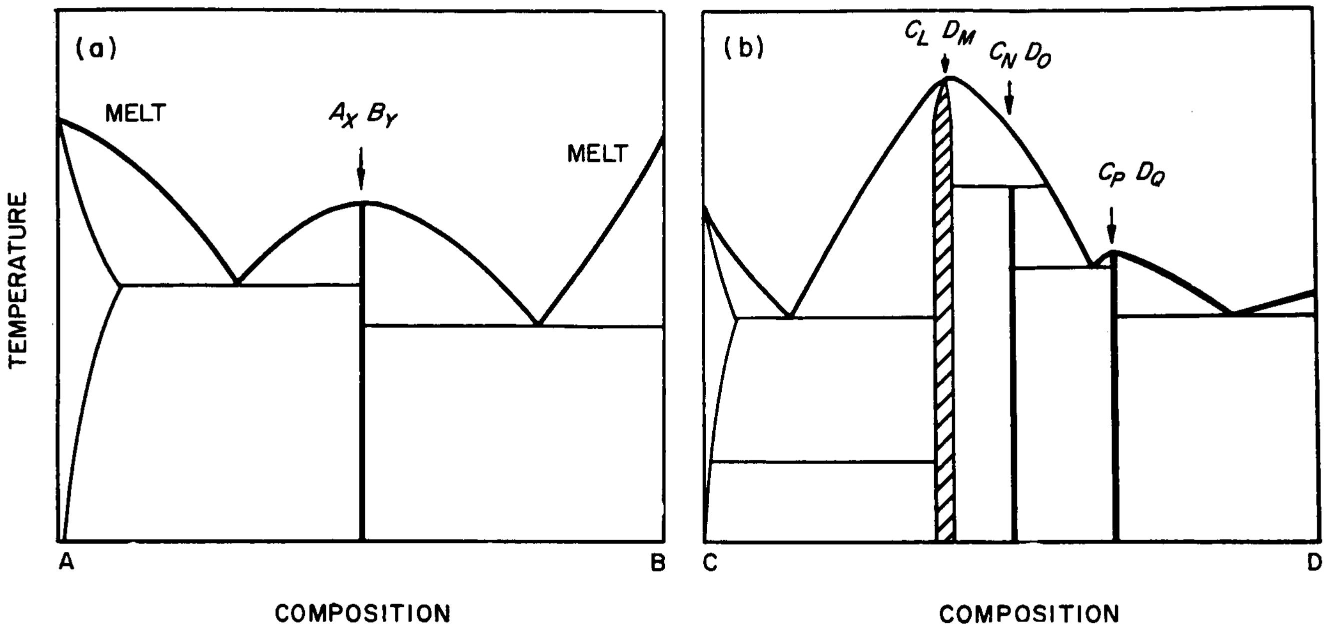

Fig. 1. Intermetallic compound defined

(a) compound A-B
(b) compound C-D

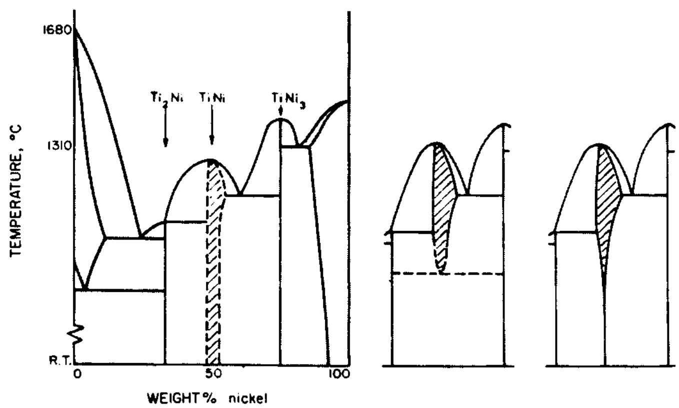

Fig. 2. Titanium-nickel constitution diagram

Intermetallic compounds have long been recognized for their unusual physical and mechanical properties. Some compounds, particularly those composed of Group III and V metals, act as semiconductors, others resist losing their strength to high homologus temperatures. However, in spite of their versatility, the usefulness of intermetallic compounds for structural application has been severely limited by room temperature brittleness. As a result, their greatest use to date has been as a minor strengthening constituent in a ductile matrix metal or alloy.

In spite of the apparent limitations of intermetallic compounds as engineering materials, the scientific demands of the U. S. Navy required a reexamination of this heretofore unpromising area. During the course of the early research on intermetallic compounds in general, one compound containing equi-atomic quantities of titanium and nickel-TiNi-showed unique properties that included marked ductility at room temperature. It is this compound and nickel-rich variations of this compound that will be discussed in some detail in this paper.

TITANIUM-NICKEL PHASE EQUILIBRIA

At the inception of the investigation on the compound TiNi and TiNi-base alloys, it was recognized that the constitution of the titanium-nickel alloy system in the TiNi phase area was uncertain. Actually, there are three versions in the literature*. This uncertainty in the TiNi composition area assisted in stimulating the metallurgists at the U. S. Naval Ordnance Laboratory to delve deeply into the alloy system.

These three versions of the Ti-Ni constitution diagram are given in Fig. 2, from left to right respectively.

INITIAL PROPERTY MEASUREMENTS

With the knowledge that near-TiNi composition alloys were (in fact) ductile, closely controlled alloys were non-consumably melted. These alloys were mechanically processed (swaged, rolled, forged, etc.) into suitable specimens and selected

*H. E. Ence Margolin and J. P. Nielsen, "Titanium-Nickel Phase Diagram," Trans. AIME, Vol. 197, 1953, p. 243
P. Duwez and J. L. Taylor, "The Structure of Intermediate Phases in Alloys of Titanium with Iron, Cobalt, and Nickel," Trans. AIME, Vol. 188, 1950, p. 1173
G. R. Purdy and J. Gordon Parr, "A Study of the Titanium-Nickel System Between Ti_2Ni and TiNi," Trans. AIME, Vol. 221, 1961, p. 636

property measurements were made. A summation of some of the physical properties is given in Table 1. These data indicate a density around 80% that of steel; melting point about 220°C below pure iron; magnetic permeability that exists stably <1.002 regardless of temperature change or plastic deformation. Further, the crystal structure was identified as cubic and the electrical resistivity was similar to the nichrome alloys. Linear coefficient of expansion and recrystallization temperature were both similar to metallic iron or nickel. There was really nothing in the general physical properties given in Table 1 to arouse interest in the TiNi-base alloys.

Table 1. Some physical properties of 55.1-Nitinol

Density	6.45 gr/cm^3	
Melting Point	1310°C (2390°F)	
Magnetic Permeability	<1.002	
Crystal Structure		CsCl (B. C. C.)
Electrical Resistivity		~80, μohm-cm
Linear Coefficient of Expansion		10.4×10^{-6}/°C
Recrystallization Temperature		550 to 650°C

Following these measurements, an expeditious mechanical property measurement was performed by monitoring hardness as a function of composition (50 to 64 wt % Ni) for "furnace cooled" and "quenched" alloys. The results of this study are shown graphically in Fig. 3. Here it can be seen that there are significant changes in hardness with minor changes in composition. In fact, this class of alloys can be divided into two major types: (1) the minimum hardness, ductile, and nonhardenable alloy containing nominally 55 wt % Ni (50 atomic % Ni); and (2) the hardenable, variable hardness, less ductile alloys containing in excess of 56 wt % Ni. For simplicity these two compositions of TiNi-base alloy will be discussed throughout the remainder of the paper. These two specific alloys will be referred to nominally as 55-Nitinol and 60-Nitinol.

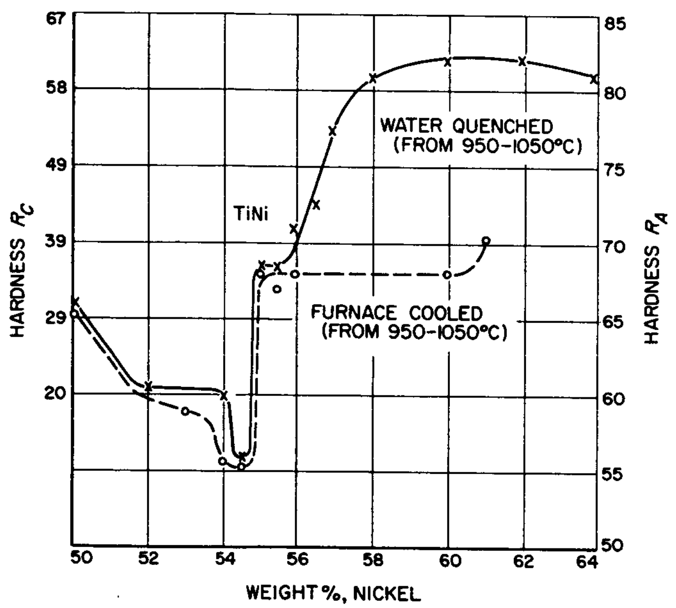

Fig. 3. Results of hardness vs percentage of nickel in titanium-nickel alloy

WHAT IS NITINOL

Because those alloys containing near equi-atomic quantities of titanium and nickel exhibit a wide spectrum of properties (see Fig. 3) a generic name was needed to easily describe specific alloys in the system. The name Nitinol, derived from Ni-Ti-NOL, was applied to this entire range of nonmagnetic alloys. A prefix numerical value (e.g., 55-Nitinol) indicates the nominal nickel content in weight percent, balance titanium.

UNIQUE PROPERTIES OF 55-NITINOL

METALLOGRAPHY

Concurrent with the property measurements, microscopic studies of the microstructure of both 55- and 60-Nitinol in various worked and heat treated forms were performed. While 60-Nitinol showed a typical two-phase structure of TiNi + $TiNi_3$, the 55-Nitinol gave anomalous results. Figure 4a reveals the typical lenticular or acicular structure found initially on all specimens regardless of prior treatment. With perfection of polishing technique, which minimized surface deformation, the true base structure of an arc-case 55-Nitinol alloy was revealed (Fig. 4b).

Based on this early metallography it became apparent that a "diffusionless" or "martensitic" crystal structure transition was possible through deformation and/or temperature change in the 55-Nitinol composition. A further criteria of this type of structure change is <u>surface mobility</u> or the gross visible distortion of a polished surface during the transition. Experimental evidence of the surface distortion is shown in Fig. 5 and Fig. 6. Figure 5 shows two hot stage photomicrographs (100X and 500X) of a plane polished surface following heating one cycle from room temperature to 100°C and cooling back to room temperature. Most of the surface distortion occurred near 40°C (probably near the M_s for this type transition). Similar evidence of surface distortion associated with this diffusionless transition is shown in the photomicrographs in Fig. 6. Here Knoop microhardness impressions were made to significantly change with one heating cycle to 100°C and return to room temperature. Repeated heating virtually eliminated the impressions entirely.

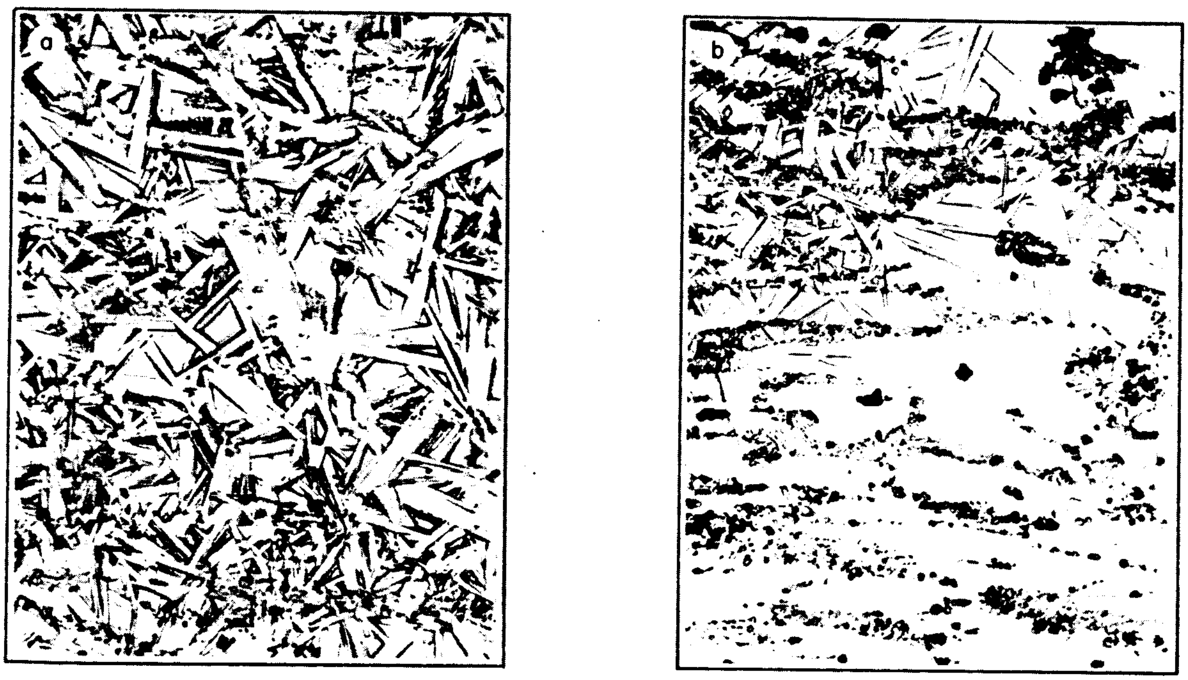

Fig. 4. Photo micrographs of titanium-nickel alloys, 500X

(a) After abrasive cutting of sample
(b) After careful surface preparation

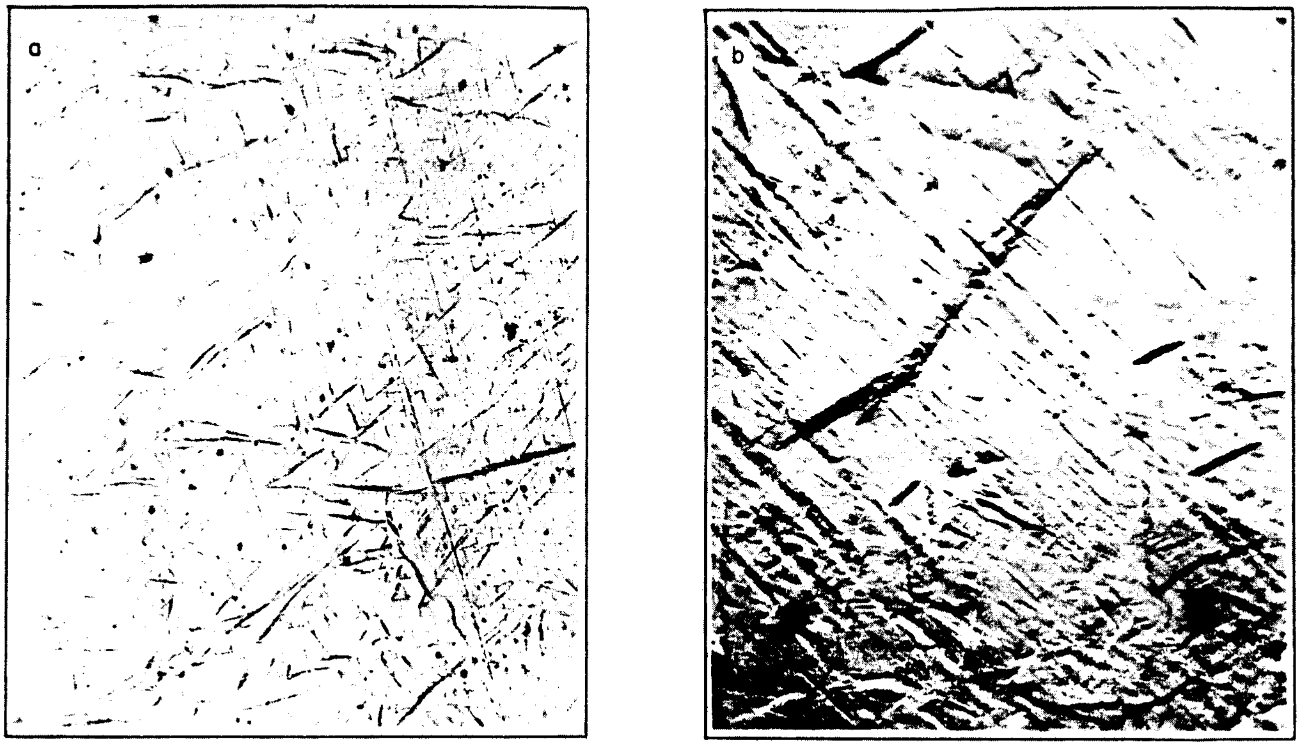

Fig. 5. Hot stage photomicrographs

a. 100X
b. 500X

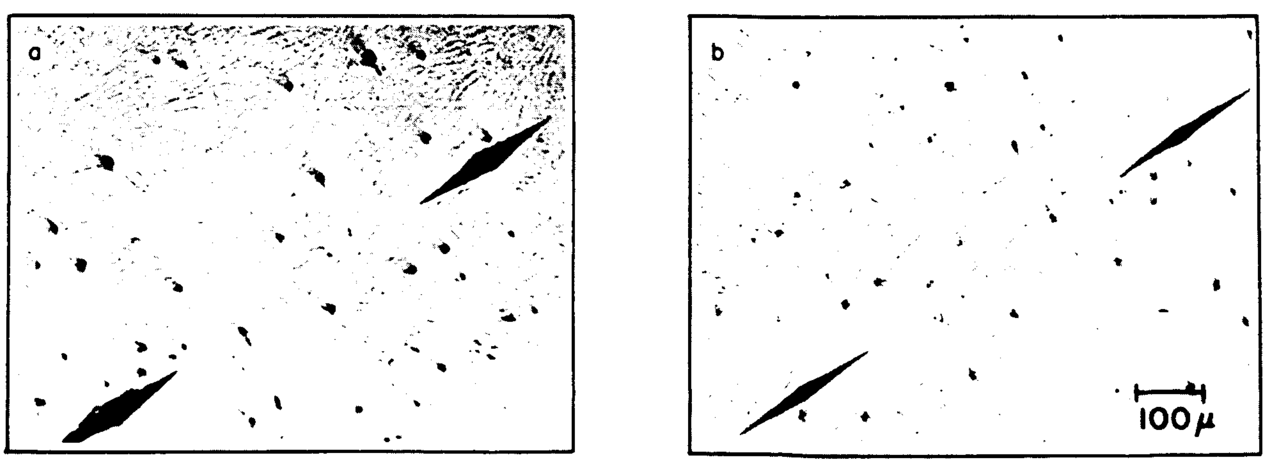

Fig. 6. Knoop microhardness impressions

a. Room temperature
b. Room temperature (after 100°C)

SOME PRINCIPAL CRITERIA FOR MARTENSITIC (DIFFUSIONLESS) CHANGE

Table 2 summarizes a few of the more important criteria* associated with a martensitic or diffusionless transition. In this table items 1 and 2 have already been demonstrated. Items 3 and 5 will be demonstrated in the film in conjunction with the oral presentation. Item 4, which states "no composition change in the transforming regions," can be implied by the very low transition temperature (~40°C in 55-Nitinol) and confirmed by X-ray crystallographic studies.**

Table 2.

Characteristics of a martensitic transformation

(1)	Martensite crystals of plate shape form on crystallographic planes
(2)	Visible distortion of a polished surface is caused by the transformation
(3)	Transformation occurs rapidly (10^{-4} sec or less)
(4)	No composition change in the transforming regions
(5)	Low temperature martensitic phase reverts back to parent phase on heating

OVERT BEHAVIOR ASSOCIATED WITH MARTENSITIC TRANSITION

This area of investigation defies verbal description; therefore, a short color movie has been used in conjunction with the verbal presentation. However, to refresh your memory on the details of this movie the various sequences of the movie are listed below.

(1) A strip of 55-Nitinol is deformed (coiled) at room temperature, heated in an air gun, and made to return to its original straight configuration.

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*Kaufman and Cohen, Progress in Metal Physics, Vol. 7, Pergamon Press, 1958
B. Chalmers, Physical Metallurgy, John Wiley & Sons, Inc., 1959

**W. J. Buehler and F. E. Wong, "Martensitic Transformations in the TiNi Compound," Proc. 5th International Symposium on the Reactivity of Solids (Munich, Germany), Elsevier Pub. Co., Aug. 1964
F. E. Wong, W. J. Buehler, and S. J. Pickart, "The Crystal Structure and a Unique Martensitic Transition of TiNi," (submitted for publication)

(2) Same strip is coiled, constrained while heated (>100°C and <400°C), cooled to room temperature, bent straight at room temperature, and heated in an air gun; now it takes coiled configuration.

(3) Wire coiled at room temperature and heated in near-boiling water; straightens rapidly showing speed of transition in less massive section.

(4) Sheet bent at room temperature transverse to rolling direction and heated in near-boiling water; reverts to straight configuration*.

(5) Same sheet as in item 4, above, bent at room temperature longitudinal to rolling direction, and heated in near-boiling water; reverts to straight flat sheet*.

(6) Series of unusual configurations and conditions to illustrate flexibility of transition.

(7) Bent rod is clamped in fixture, heated at the bend, and bears against wooden pencil mounted as a simple transverse beam; pencil bows and fractures under force of transformation. A minimum force of 35 lb is required to fracture pencil under above conditions.

(8) Drastic change in damping is demonstrated by striking a freely suspended 55-Nitinol arc-cast bar at room temperature and when heated a short time in water near the boiling point. Bar changes from highly attenuating condition (a leaden thud) at room temperature to a very low attenuation (bell-like ring) at the near-boiling water temperature. This behavior is shown more quantitatively in Fig. 7 where the decay in torsional vibration is plotted as a function of the number of cycles. Most significant changes appear to occur between room temperature and about 65°C. For the alloy composition concerned, this should cover the martensitic critical M_s temperature.

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*Illustrates isotropic nature of transition in structure.

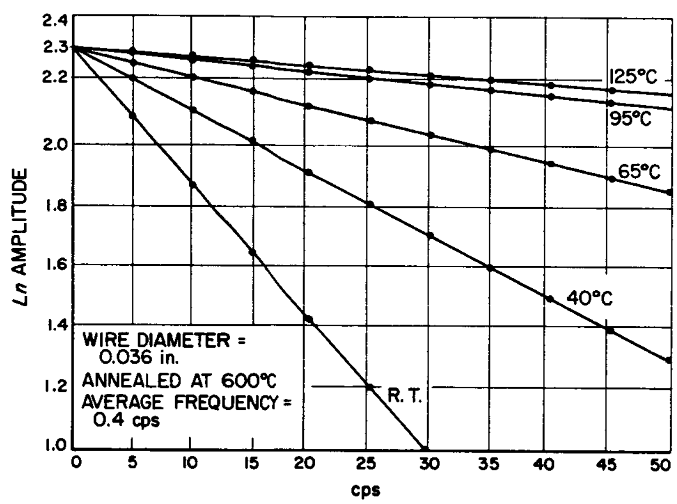

Fig. 7. Decay in torsional vibration as a function of number of cycles for 55.4 wt % Ni-Ti alloy wire

With the drastic vibration damping change as a function of temperature there appeared to be a direct relationship between the damping transition and the M_s critical temperature. Arc-cast bars of varying composition were made and qualitatively tested for their damping transition. The resultant plot is shown in Fig. 8. This curve shows a maximum transition temperature near the equi-atomic alloy (nominally 55-Nitinol).

Further, if an alloy of 56-Nitinol is prepared in wrought form, it must be cooled well below room temperature (see Fig. 8) to have good ductility. At room temperature this alloy will be very stiff and resist plastic deformation. Also, a wrought piece of the 56-Nitinol, when deformed at the low temperature, will recover its room temperature shape on returning to room temperature. From this experimental evidence it appears the activation temperature of both the damping and configuration changes can be adjusted by carefully manipulating the alloy composition.

60-NITINOL

In the filmed sequence on the hardenable 60-Nitinol three points were illustrated.

(1) A hardened 60-Nitinol cutting edge was used to sever a common steel nail. It could be seen that there was no damage to the cutting edge. Showing conclusively that 60-Nitinol cannot only be made hard (up to 62 R_c) but it has sufficient toughness to withstand rigorous tests without failure.

(2) Using a sensitive magnetometer it can be seen that hardened 60-Nitinol (or all Nitinol alloys) is absolutely stably nonmagnetic. Comparative runs were made with a currently-used copper-base nonmagnetic tool and a steel screw driver.

(3) Pivot bearings (four ball bearings per race) and a U. S. Navy swimmers' knife, both made of hardened 60-Nitinol, are shown to illustrate the complexity of fabricated parts possible from the alloy.

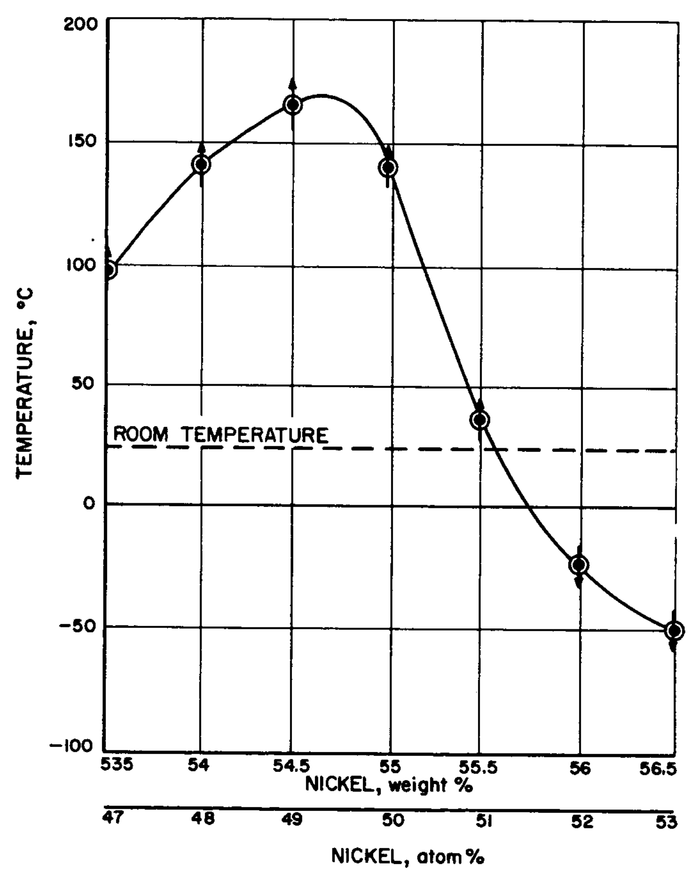

Fig. 8. Damping transition curve

HARDENABLE 60-NITINOL

Earlier in this paper Fig. 3 was given to show that there was a marked change in the Nitinol alloys as the composition is varied. Further, in Fig. 3 we have seen that a nominal 60-Nitinol alloy has a thermal hardening capability greater than other known nonmagnetic alloys (e.g., Haynes-25, Be-Cu, etc.). Additionally, this alloy (see Fig. 3) has a potential range of hardness from near 30 R_c to 62 R_c depending on the rate of cooling (from 900 to 1000°C) or the conditions and amount of "overaging" applied to the "quench-hardened" material.

What then is the mechanism of this hardening process and how stable is the hardness? Figure 9 can best be used to answer this question. In spite of a disagreement on the phase equilibria in the low temperature reaches of the TiNi-phase area (gray area in Fig. 9a) there exists a uniform agreement about the retrograde solubility change between the TiNi and TiNi + $TiNi_3$ phase regions. Using this solid solubility change with temperature and the general "lever-relationship" as applied to phase quantity, it can be seen how with heating (see balloons, Fig. 9b) much excess nickel is taken into solid solution by the TiNi phase. On cooling rapidly from above about 900°C, the excess nickel in the TiNi phase must precipitate as $TiNi_3$ and does so in such an effective manner as to strain and harden the TiNi matrix to tool steel hardness.

Slower cooling rates will result in coalescence of the fine $TiNi_3$ precipitate, at the higher temperatures, into less effective hardening particles. Very slow rates of cooling result in a two-phase alloy of large innocuous particles of $TiNi_3$ in a TiNi matrix. This alloy has a hardness not too much higher than that of the 55-Nitinol. Drastic quenches are not required to attain maximum hardenability. 60-Nitinol has been found to have a hardenability (Jominy End-Quench) superior to AISI-4340 steel.

Another means of providing a lowering of the 60-Nitinol hardness is through "overaging." This is done by quenching the material to maximum hardness followed by heating for times and temperatures between about 600 and 850°C, followed by a furnace cool. The time-temperature relationship is a controlling factor in this 'psuedotempering" process.

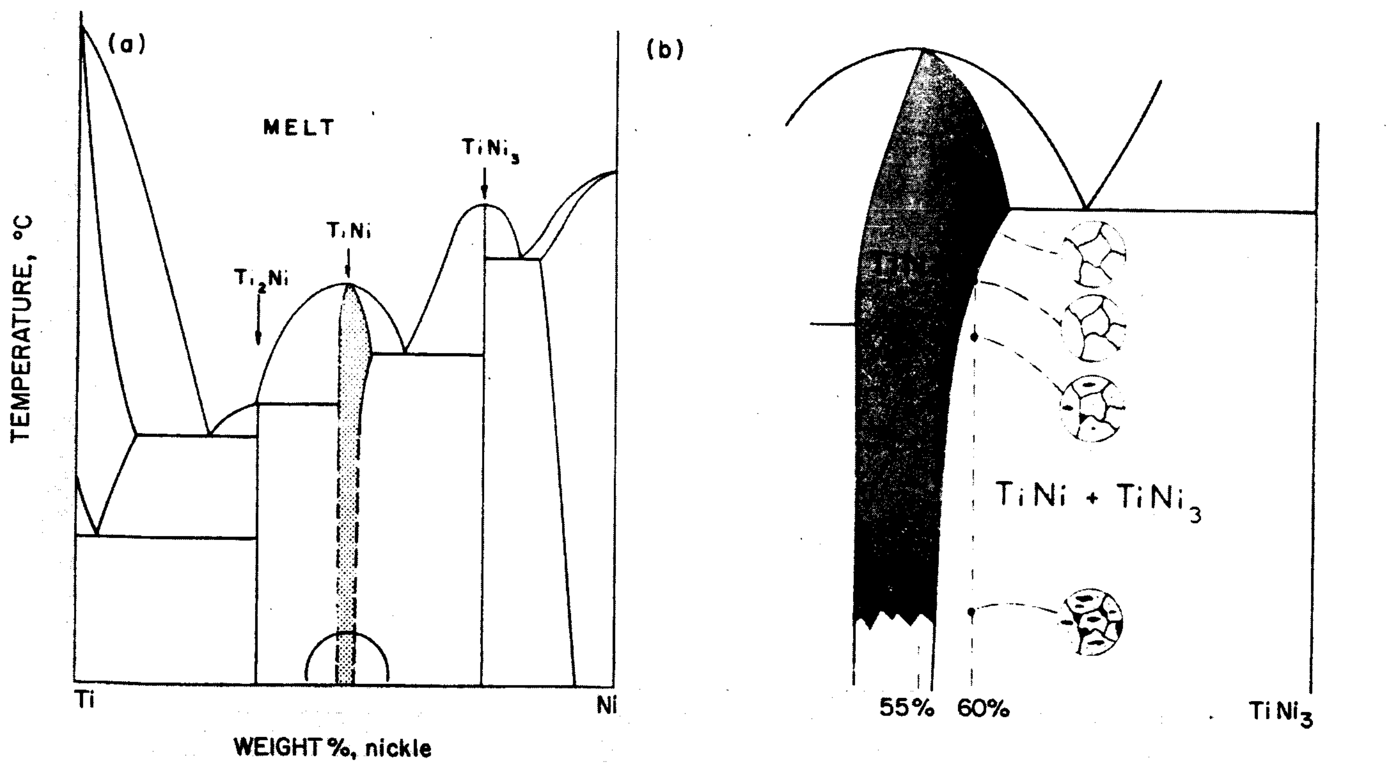

Fig. 9. Titanium-nickel constitution diagram showing Nitinol-60

(a) Showing low temperature reaches
(b) Detailed of shaded area

FABRICATION

In light of the entirely different properties inherent in each of the alloy ranges 52- to 56-Nitinol and 57- to 62-Nitinol, it must be recognized that the fabrication procedures in these alloy ranges will vary. Figure 10 shows the direct conventional hot and cold working procedures possible on the arc-cast 52- to 56-Nitinol and the resulting microstructures. Alloys in the hardenable range (57- to 62-Nitinol) require one additional processing step. That step is hot extrusion or the like (e.g., press forging, etc.) to "break down" the arc-cast structure. Observing the right-hand set of photomicrographs in Fig. 10, the $TiNi_3$ phase can be seen (light gray) in various shapes, quantity, and distribution depending on the working and heat treatment.

From an overt standpoint Fig. 11 shows both alloy ranges represented as wrought products. In the successful working of the Nickel-rich alloys (57- to 62-Nitinol) two basic ideas must be observed: (1) the arc-cast billets must be "broken down" properly, in this respect they are similar to the arc-cast refractory metals, and (2) subsequent hot working should not be attempted above about 950°C. Figure 12 shows a typical example of upset press forging of a 61-Nitinol arc-cast ingot. The original conditioned ingot was upset from 7.5 in. diameter by 3-1/4 in. to 9 in. diameter by 2-3/8 in. at a 1625°F metal temperature.

Joining the Nitinol alloys has been given only preliminary consideration to date. Figure 13 shows the fusion section of an arc-welded butt joint in 55-Nitinol. Though no quantitative data are available on fusion welded joints, certain observations were possible.

(1) With the high titanium content of the alloy some form of shielded or protective atmosphere must be present during welding to prevent gaseous (O_2, N_2, H_2) pickup and embrittlement of the welded joint.

(2) The microstructure of the fusion zone shows a fine dendritic pattern. Based on the tensile properties of high-purity arc-cast specimens, 65 to 75% of the ultimate strenght of the wrought material should be realized. Further, clean welds (particularly low in oxygen) should have fair ductility and impact properties.

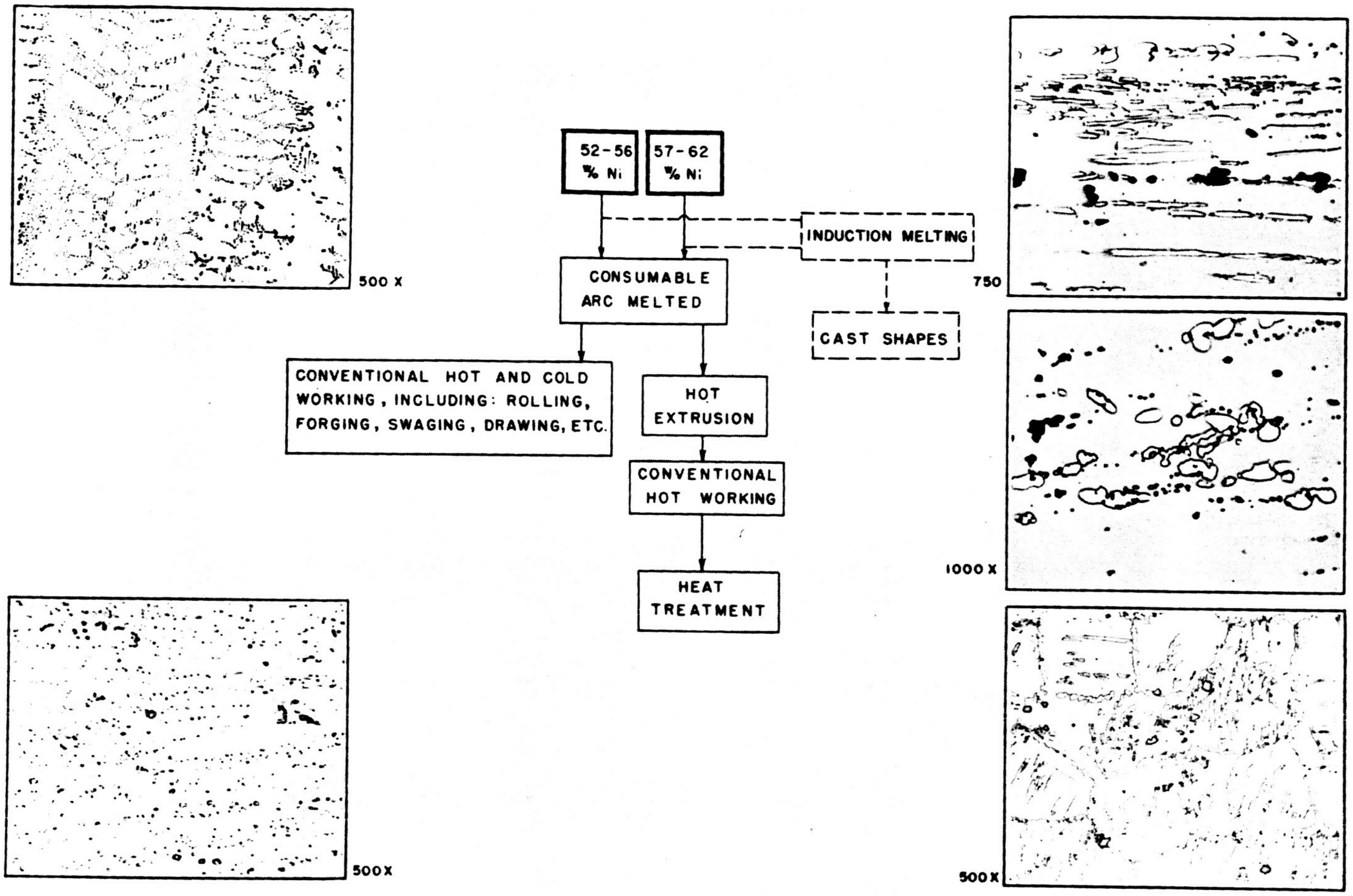

Fig. 10. Nitinol fabrication

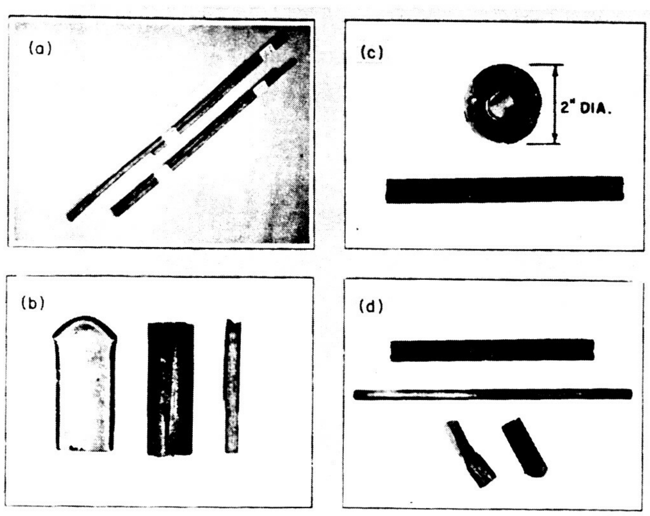

Fig. 11. 52- to 56-Nitinol and 57- to 62-Nitinol shown as wrought products

a. 55.1- and 56-Nitinol, commercially prepared bar stock
b. 60-Nitinol, hot rolled
c. 60-Nitinol, billet end and hot extruded rod (10:1 area reduction)
d. 60-Nitinol, hot extruded rod and hot swaged, forged, and rolled specimens made from this rod

Fig. 12. Press forging of 61-Nitinol arc-cast ingot

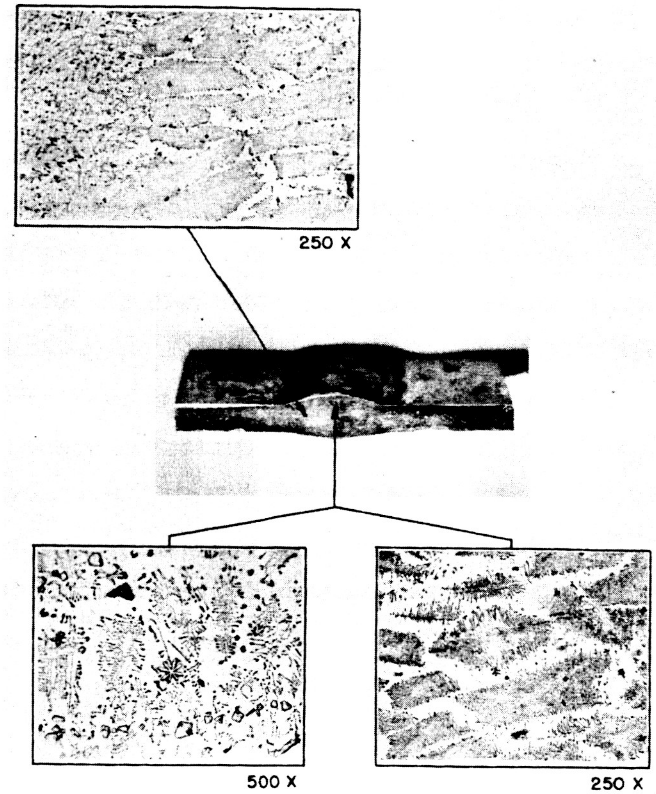

Fig. 13. Heliarc welded Ti Ni alloy plates (2X) (Photomicrographs of portions of weld section are also shown)

Spot welded 55-Nitinol is shown in Fig. 14. Experimenting to attain best welding conditions results in a typical shear failure where a nugget is torn from the joined strips. In spot welding very fine 55-Nitinol wire (< 0.005 in. diameter) it was found desirable to protect weldment with an inert atmosphere (Helium, Argon, etc.) during spotting. The inert gas served to prevent the embrittlement of wire areas adjacent to the weld by minimizing massive oxygen pickup.

PROPERTIES

Some typical mechanical properties for both 55- and 60-Nitinol are given in Table and Table 4. Based on these tables and other work, certain observations are possible.

1. For an intermetallic compound, the TiNi material (55-Nitinol) possesses a very high ductility (22% tensile elongation). This is further confirmed by the high charpy impact strengths (Table 4.)
2. Further, while recognizing much of the observed ductility is probably related to the martensitic transition*, the very high (sometimes higher) impact strength at -112°F implies transition kinetics down to this temperature and even lower.
3. Sharp-V notched charpy specimens have also been tested. These data show that both 55- and 60-Nitinol are notch-sensitive. As a result, this must be made a design consideration.

Very recent work at the U. S. Naval Ordnance Laboratory on the cold deformation of 55-Nitinol indicates a capability for raising the yield strength of this alloy many magnitudes without seriously lowering the tensile elongation. While this processing technique virtually eliminates the unique damping and dimensional changes possible in the alloy, it opens a new area in ultra high strength nonmagnetic alloys.

Some work has been conducted on the fatigue properties of the 55- to 56-Nitinol alloys. These data reveal unique fatigue capability, e.g., a rotating beam 56-Nitinol specimen (Fig. 15a) ran 25×10^6 cycles under a load of 70,000 lb/in.^{2} without failure. Further, the specimen was continuously bent with a deflection (d) equal to 1/8-in. under the 70,000 lb/in.^{2} load. This deflection is depicted in Fig. 15b. Why was the specimen able to withstand 25×10^6 cycles under these conditions and not fail or show any sign of heating? The answer must again be

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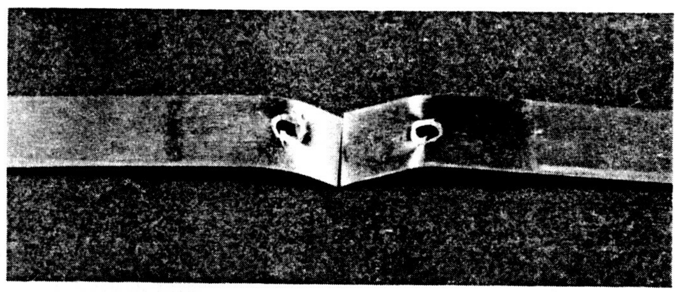

Fig. 14. Spot welded Nitinol showing nugget torn from strips during typical shear failure

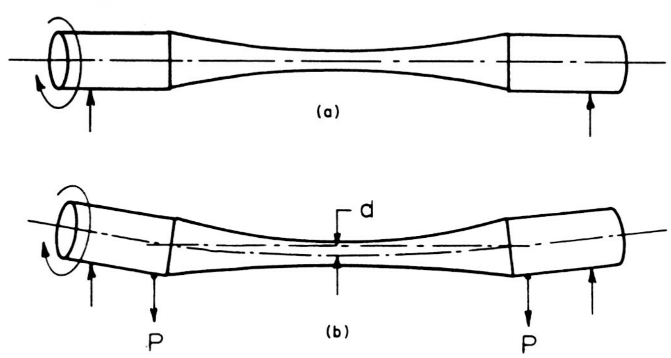

Fig. 15. Fatigue test samples

a. Without deflection
b. With deflection

Table 3. Tensile properties (strength)

Composition, percent nickel	Specimen condition	Specimen hardness	Tensile strength, lb/in^2	Total elongation, percent	Young's modulus, lb/in^2
55	Hot worked	20 R_c	125,000	22	10×10^6
60	Extruded (hot)	50 R_c	178,000	<1	15.3×10^6
	Quenched	61 R_c	154,000	<1	16.5
	Furnace cooled	37 R_c	137,000	7	14.2

Table 4. Impact properties (toughness)

Composition, percent nickel	Specimen condition	Testing temperature	Impact, ft-lb
55	Hot worked	Room Temperature	117
		-112°F	70
55.1 (plus up to 0.08 iron)	Hot worked	Room Temperature	153
		-107°F	160
56	Hot worked	Room Temperature	105
		-112°F	103
60	Extruded (hot)	Room Temperature	56
	Hardened (60 R_c)	Room Temperature	38

related to the unique martensitic transition and how this crystal structure transition is affected by various modes of deformation. Further work is underway to better understand this relationship.

The Nitinol alloys are resistant to the corrosive action of many common media. Strong acids and bases of mixed acids will attack the system. However, in an air atmosphere or under severe marine conditions, the Nitinol alloys are excellent. Currently, tests of jet-impingement, cavitation-erosion, etc., are in progress at the Marine Engineering Laboratory, Annapolis, Maryland. These tests

to date have shown both 55- and 56-Nitinol to possess outstanding resistance to sea water. The entire marine corrosion program will be completed by the summer of 1965.

APPLICATIONS

Based on the properties of the Nitinol alloys, the potential applications are numerous. Table 5 divides these applications in two major types on the basis of composition. Most of the 55-Nitinol applications are related in some way to the unusual vibration damping and dimensional changes that are possible because of the diffusionless or martensitic transition. The characteristics of being nonmagnetic and corrosion resistant are accidental additional benefits. On the other hand the Nickel-rich TiNi alloys (56- to 62-Nitinol) have applications that stem mainly from their combination of nonmagnetic, hardenable, and corrosion resistant properties. This latter group of alloys provides, for the first time, a decent material for making good quality nonmagnetic tools, bearings, and other components.

In the general area of space application 55-Nitinol appears to have useful potential in storing energy for subsequent use in doing work in space. More specifically, it appears the fail-proof natural physical phenomenon related to the structure changes in 55-Nitinol might be used to perform self construction tasks, critical movements, etc., in space by exposure to heat from solar radiation, chemical, or electrical sources. Further, the 56- to 62-Nitinol alloys appear to answer the need for those critical nonmagnetic components where hardness, strength, and abrasion resistance are required.

OPEN DISCUSSION

MR. ENGLAND: England, Westinghouse.

You mentioned that the ultimate strength for the 55% nickel compound was 125,000 lb/in^2, but I missed the yield strength. Could you repeat that?

MR. BUEHLER: The yield strength will vary as a function of temperature, but at room temperature it is on the order of 10,000 to 20,000.

Table 5. Potential applications

Composition	Application	Property						
		Nonmagnetic	Hardening capability	Lower density	Corrosion resistance	Vibration damping	High strength	Diffusionless transformation (deform. -temp.)
55-Nitinol	Low noise structures			X	X	X		
	Temperature sensing devices	X			X			X
	Armor			X	X			X
	Outer space components	X		X	X			X
56- to 62-Nitinol	Nonmagnetic hand tools	X	X				X	
	Nonmagnetic bearings	X	X				X	
	Instrument components	X	X		X			
	Underwater ordnance	X		X	X		X	
	Chemical handling equipment				X		X	
	Cutlery		X		X		X	
	Hydrofoils			X	X	X	X	

VOICE: Are there any glasses that this will match in coefficient?

MR. BUEHLER: I would think not, just as a quick answer. As you can see, the 55-Nitinol is dimensionally not too stable; the 60-Nitinol, on the other hand, is quite dimensionally stable. Even there, I think that the coefficient of expansion is probably a little high for most glass material.

MR. BROOK: We have a current problem that I mentioned here before, on an X-ray telescope. One of the problems has been that the experimenter would like to use a material called 320-stainless, and one of the reasons he likes it is due to its very tight crystalline structure that is able to reflect X-rays. Now, could this material be used as a substitute? We have been looking for one rather desperately.

MR. BUEHLER: Well, this is a body-centered cubic material, and if his stainless is 320, it is essentially a cubic-type material. I would say that they would have similar characteristics in this respect. If his definition of a tight structure is what he is really looking for, I think he may have a possibility of substituting this for the 320.

PREFERRED PARTS AND MATERIALS

E. J. Iufer
Ames Research Center
Moffett Field, California

I am not going to follow the plan exactly as I conceived it before I arrived 3 days ago. I find that you cannot just use a parts list for all the different programs that are represented here as being "the parts list." I would like to go into some of the background thinking that you will have to apply to select parts for your program.

We will be discussing some of the basic considerations in setting up screening and acceptance testing procedures for parts and assemblies. We will also be summarizing the basic principles for parts and material selections, and fabrication techniques to achieve low magnetic moment.

The overall magnetic program for Pioneer is based on the requirements of the magnetic mission, and has had a very significant impact on spacecraft development schedules and costs. When so much is involved, it is important that testing procedures be accurate.

During the course of magnetic screening tests of raw materials used in the manufacture of electronic parts, it has been found that the previous magnetic history of these parts varies widely. For example, we have found that some manufacturing processes use permanent magnets to transport electronic parts. Consequently, to standardize the magnetic tests for remanence on any given specimen, it is important that the previous magnetic history be removed and a known exposure be given prior to the test.

Our experience to date indicates that the perming exposure should start at at least 25 gauss. The deperming exposure should probably start at at least 50 gauss. In certain instances, it has been necessary to go way beyond 100 gauss to erase remanence.

If you talked to either Goddard or Ames, you would find that we have one line that we keep preaching; that is, to measure everything as often as you can and, after you have the information, to do something with it. I can illustrate this by one experimenter who brought a specimen, and it was very magnetic, and we found the shell on one connector created a magnetic contamination that was in itself three times the allowable specification for the entire instrument. It seems that they had to assemble the connector just before shipment to us, the clean magnetic parts were in a bonded

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storage, the technician on that shift did not have the key to the storage, and so he used a magnetic type.

This means you can't assume that the measurements you have on a screening inspection are going to be adequate. Every time something happens to the box that could possibly change its magnetic characteristics, and I am talking essentially about the replacement of parts, you should remeasure to be sure that it hasn't become contaminated.

I would like to talk a little bit about the deperming process as it would be used for, perhaps, screening inspection or small component subassembly measurements. We found that the deperming process requires attention both to control of the DC ambient field and to the level of the AC demagnetization field. We found that during the deperming process the ambient magnetic field should be in the order of 1 millioersted or less to prevent a resulting horizontal shift in the hysteresis loop that would result in a residual induction. This corresponds to something more than a two order magnitude reduction in Earth's field.

This can normally be done with a simple Helmholtz pair, and has been done, in some cases, with a large, single annular coil properly placed so that its axis is parallel to Earth's total field vector. We found that the decrement between the pulses used in the deperming sequence should be no greater than 2 gauss. This means that if you were to give a 25-gauss exposure, 12 shots would probably give you a satisfactory deperm. By satisfactory deperm, we feel that you should get at least a 10-to-1 improvement from the remanence corresponding to 25-oersted exposure.

Where the deperming operation was not successful using this procedure of compensating out Earth's field and using 10 to 15 pulses from 25 gauss down to zero, increasing the number of shots or decreasing the decrement between shots did not remedy the situation. We found, essentially, that we had to always start with a higher level in the demagnetization process.

One of the tricks to do this is to have the specimen in a low field area and use a handheld AC source, such as the growler that I mentioned, or a simple coil of wire that is driven by a Variac. By mapping the field of this device, you can quite accurately determine what level of AC you are using. In the case of a growler, you can go up to 1000 gauss at the surface of the laminations.

In general, it has been found that the coercive force of the material used on the Pioneer experiments was such that if you expose a specimen to a 25-gauss field, remove the field, then reverse the polarity and give the same specimen a reversed

exposure of 12 gauss, and then turn that off, you find that the reversed 12-gauss exposure will essentially cancel the effect of the 25-gauss exposure. This can be visualized, by a doubling of the efficiency of the deperm operation.

This is to give you some judgment on what happens to the magnetic remanence left by a standardizing exposure because of manipulation of the specimen in Earth's field. Because Earth's field is in the order of 0.5 gauss, and if this effect is doubled, you have 1-gauss influence; so that you can expect a change in the magnetic remanence, post 25 gauss of something like 1 part in 25, just because of possible handling of the specimen; not that 4% is a significant number, but that if you use a standardizing exposure of 2 or 5 gauss, the relative effect that Earth's field could have, would be very significant.

The equipment that we use for deperming is very simple. We have a 4-ft Helmholtz coil that requires approximately 11 amperes to produce 25 gauss. We have a commercial power supply, a reversing switch, and we use the magnitude controls right on the face of the power supply. It is a matter of switching to one polarity, turning current up to a given value, turning it down, switching to the next polarity, and so on. This takes approximately 1 min for the treatment.

I would like to talk a little bit about this magnetizing exposure for accepting experiments on Pioneer. This, essentially, is the basis for the acceptance. The standard exposure should be very carefully controlled. This means that one should know the magnitude of the field applied to the specimen, not only at the center where it is commonly measured but also at the edges. The rule of thumb here is that if the diameter of a Helmholtz coil is at least twice the longest dimension of a specimen, the error because of the nonuniformity in the Helmholtz coil will be significantly below the overall system error.

We find that it is very important that one has accurate angular positioning while exposing a specimen. We found that the tumble-perming technique does not produce consistent results. We can visualize a certain black box being permed vertically upwards, we leave the field on and slowly rotate the box about a horizontal axis 120 deg, and then we turn off the field and withdraw the box; we have essentially permed it to the 25 gauss parallel to this axis, and we have gone past zero and we reached the sine of 30 deg in the other direction. So the perm you initially set into the first axis, you now have canceled just before you withdrew the box.

So if one word can characterize the properties of a black box; it is the anisotrophy of its magnetizations. A rule of thumb is to give exposures under

completely specified circumstances. For individual parts, the exposure should be given parallel to the long magnetic axis of the part. For transistors, diodes, this means that you need to make only one measurement, one exposure parallel to the leads. For more complicated assemblies such as entire instruments, it is necessary to give a three-cycle operation of a consecutive perming, deperming, perming on each of the axes.

I think perhaps we ought to discuss the overall error when one makes measurements. We have found that, by and large, for nominal measurement distances, you cannot rely on the inverse cube rule. For boxes having a major dimension of 8 in. and a measurement range of 18 in., one might underestimate the contribution of this box at a 6-ft magnetometer range by, perhaps, 14%. If you measure closer, this error becomes even greater.

So you are essentially trying to compromise the extreme long range of measurement where you would be, perhaps, in the dipole field against being close enough to the box so the signal you are actually measuring is significantly above the noise level, so that the noise level isn't producing the major amount of error. For Pioneer, we have established an acceptance range during measurement of 3 ft. The specification is based on the measurement at 3 ft, rather than a closer measurement that is extrapolated by some specific power to 3 ft.

Besides the error in making inverse cube extrapolations, there is also a range error. If you are measuring a simple dipole at a range where the distance between the two poles of the dipole were negligible, you would find that the percentage error in determination of its moment would be something like three times the percentage error in the measurement range.

On higher order multipoles you find that this goes on up. For a quadripole the error is 4%, and so on. This is to alert you not to measure too close because the exact center of a lot of samples may not be very easy to define, so one doesn't know the exact point to use for referencing his measurement.

One of the most effective methods to eliminate induced magnetizations from measurements of remanence is to either use two magnetometers or Earth's field compensation. For Pioneer hardware, the ambient field of 1 gauss produces an induced moment roughly equal to the remnant moment after a 25-gauss exposure. This means that Earth's field would only need to be reduced by a factor of 50, so that the concurrent induced moments during remenant moment measurements would be negligible. And by "negligible," I mean approximately 1% or less.

You have heard quite a bit about the selection of parts and materials for nonmagnetic spacecraft from the previous speakers. These general principles apply equally to Pioneer. In our case, in general, the policy is to use no part when a less magnetic substitute is suitable.

It is also very important to pass on that nonmagnetic restriction to the lowest level, to the part vendors themselves. We have also found that industry is becoming receptive to design change to improve the low magnetic characteristic of their product. One of our experimenters planned to use some 230 polar solid tantalum capacitors, and it didn't take much of a calculation to find that if he were to use the standard nickel-leaded capacitors, that he would not come close to meeting the magnetic specification. So we, of course, considered clipping the leads. Figure 1 shows the improvement you can get by simply clipping a lead.

This is a 350D-style capacitor with standard nickel leads, and at the far right you will find leads somewhat over 1-in. long, and the magnetic moment has just been normalized on a log scale to give you an idea of the percentage of the difference. You can see that going down to approximately 50% lead length, you get one order of magnitude improvement. By cutting 1/2 in. off of a lead, you could have 10 times as many parts as with 1-in. leads.

Now, if you went from a 1/2-in. lead to a 1/4-in. lead, you now could have 100 parts, where you could only have one with the original lead configuration.

Finally, by clipping less than 1/4 in., you wind up with something approximately of two orders of magnitude of improvement. The magnetization that you are looking at is almost 100% because of the magnetic material used in the lead.

The magnetic contribution of this many capacitors, even with lead clippings, was somewhat overwhelming. So we started to try and produce a source of nonmagnetic capacitors and, after a search not unlike the one that JPL discussed this morning, we finally found a manufacturer that produced a quantity of some 2500 capacitors that fulfilled the requirements of a Hi-Rel Military Specification, MIL-C-39003/01.

This capacitor is a polar solid tantalum. It has a sintered tantalum core. All we specified was that it have nonmagnetic leads; it still has a metal-to-glass seal. It was designed as follows.

The button in the end is a standard glass header; however, instead of fusing the core or the lead into the center, it has a little eyelet made of Kovar and it also has a little band around the outer rim of this header made of Kovar. The lead

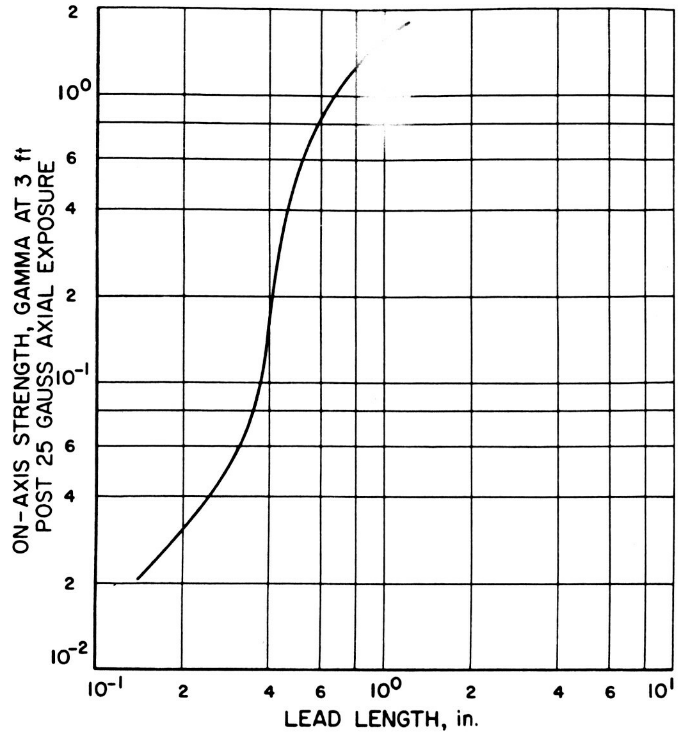
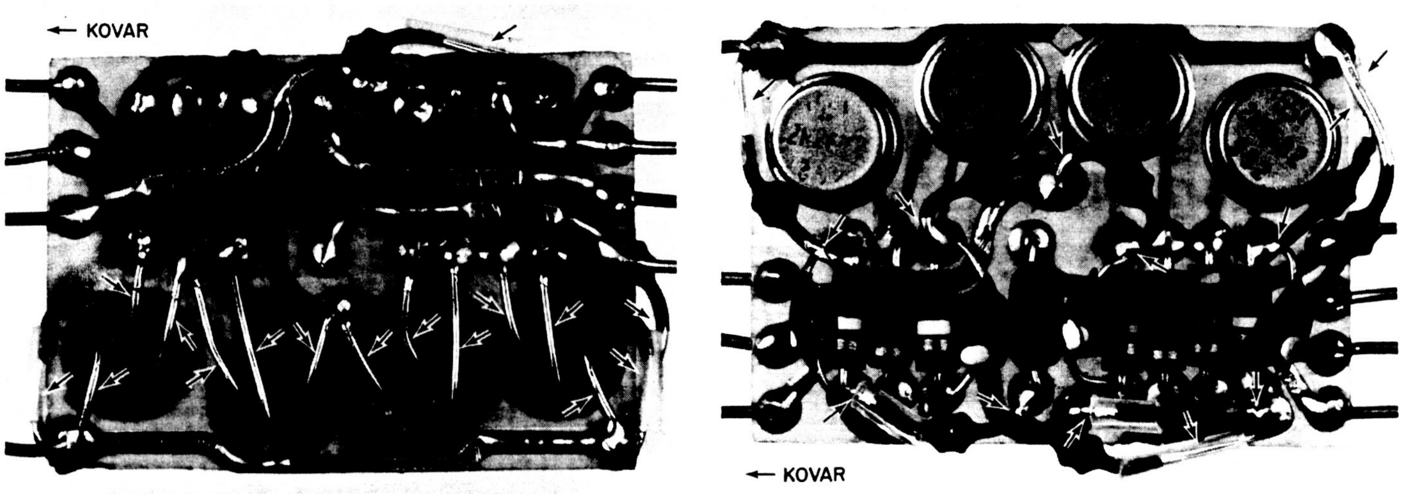


Fig. 1. Effect of lead length on magnetic moment of typical solid tantalum capacitor (MIL-C-26655A, sizes A-D)



a. Bottom view b. Top view

Fig. 2. Cordwood module with Kovar transistor leads

material and the can material is Alloy 180. The lead is passed through this little eyelet and soldered in place, then the outer rim of the header is soldered to the can.

Every sample was tested for leak by putting it in a 125°C oil bath and watching for bubbles. No part failed; 100% passed these standard tests for MIL-C-39003/01.

On magnetic screening of the parts at Ames, we found that we had made a three orders of magnitude improvement with full lead lengths compared to the 350D capacitors.

Incidentally, we found that, of the 2500, one capacitor with nickel leads had been included in the same lot number. It was very magnetic. This points up that even if you have Government source inspection, you can't be assured that you are going to be completely free of nonconforming material.

As a general policy, clip off all unnecessary lead material. The transistors used on Pioneer are specified to 3 gammas at 3 in. after a 25-gauss exposure and after the leads have been clipped to approximately 1/8 in. Some photographs will illustrate how you fabricate using cordwood construction.

Figure 2a is an example of how not to build a cordwood module. This is a closeup photograph of a module used in one of the experiments as originally proposed. The actual dimension of this is about 1 in. across the top and 1 in. high. The Kovar material is indicated by arrows in Fig. 2. The objection to this Kovar material is that in one case, near the upper left-hand quadrant, a transistor lead has been draped diagonally to the right and diagonally downward clear to the opposite corner of the module. This module was checked and found to be too magnetic to meet the overall objective of the instrument.

By remarking the drawings, new art work was designed that moved the pads right to the base of the transistors so that it was not necessary to use transistor leads longer than about 1/16 in.

Magnetic design must start at the beginning of the layout of the assembly; otherwise, the designers will design the art work to eliminate crossovers, to make it easy to draw, or to make it easy to assemble. These approaches may conflict greatly with good magnetic design.

Figure 2b is a top view of the poorly designed cordwood module. Here the Kovar is associated with the diode's leads. Because they are draped around without any particular attention to clipping of leads shows bad practice.

Figure 3 is a photomultiplier that was supposedly delivered as nonmagnetic; for other reasons it was a test model without envelope, and we were able to identify

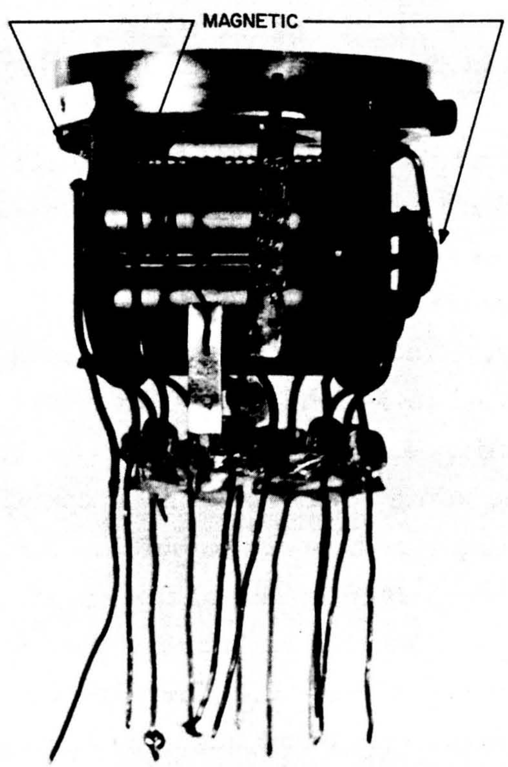


Fig. 3. Photomultiplier tube without envelope

magnetic materials still used in this assembly in the seals at the bottom and other points shown in Fig. 3. We use a very simple instrument to find small magnetic masses. This instrument consists of a magnetized darning needle glued to a silk string so that it is balanced horizontally.

We found that we can magnetize a needle so that it does not expose the test specimens to field intensities greater than 25 gauss. By passing this needle over the test specimens we are able to locate magnetic material as small as a grain of salt. This technique was used to locate the magnetic materials found in this supposedly nonmagnetic photo-multiplier.

The manufacturer of this multiplier tube was very successful in reducing the mass of magnetic material used for leads. This was done by welding Alloy 180 to the Kovar seals, inside and out. This tube now contains practially no Kovar except right in conjunction with the glass.

It is important that, if one is going to use a coil facility, one must be free of the ambient magnetic noises associated with industry. The small dot in the center of Fig. 4 is our magnetics laboratory. We have about a 1/2 mi perimeter of nothingness.

Figure 5 may be of interest to people who would like to sort of follow the design development of modules. Besides laying out stiff specifications, we also provide a design and consultation service. Here, the experimenter was trying to reduce the magnetic moment of his module and he sent us a module; then we would tell him what the magnetic moment of the module was in each of the directions, which he then could sum up by the number of modules and predict the overall field of the experiment.

Across the top we pasted on Polaroid shots of the actual model for identification purposes. It is very easy to lose tags and forget what a code number might represent in hardware, but a picture is concise. From the chart, we see the general trend of improvement.

You will have a lot better success dealing with the experimenters or with designers when you can show them graphic results of good magnetic design.

Figure 6 is a replica of a recording off our X, Y plotter. Essentially, the technique is to take a picture of the experiment, which records the identity of the unit and the measurement coordinate system. The axis from left to right is azimuth angle, and has the scale of 4 deg/0.1 in. The up and down axis is field magnitude and has the scale of 0.1 gamma/0.1 in.

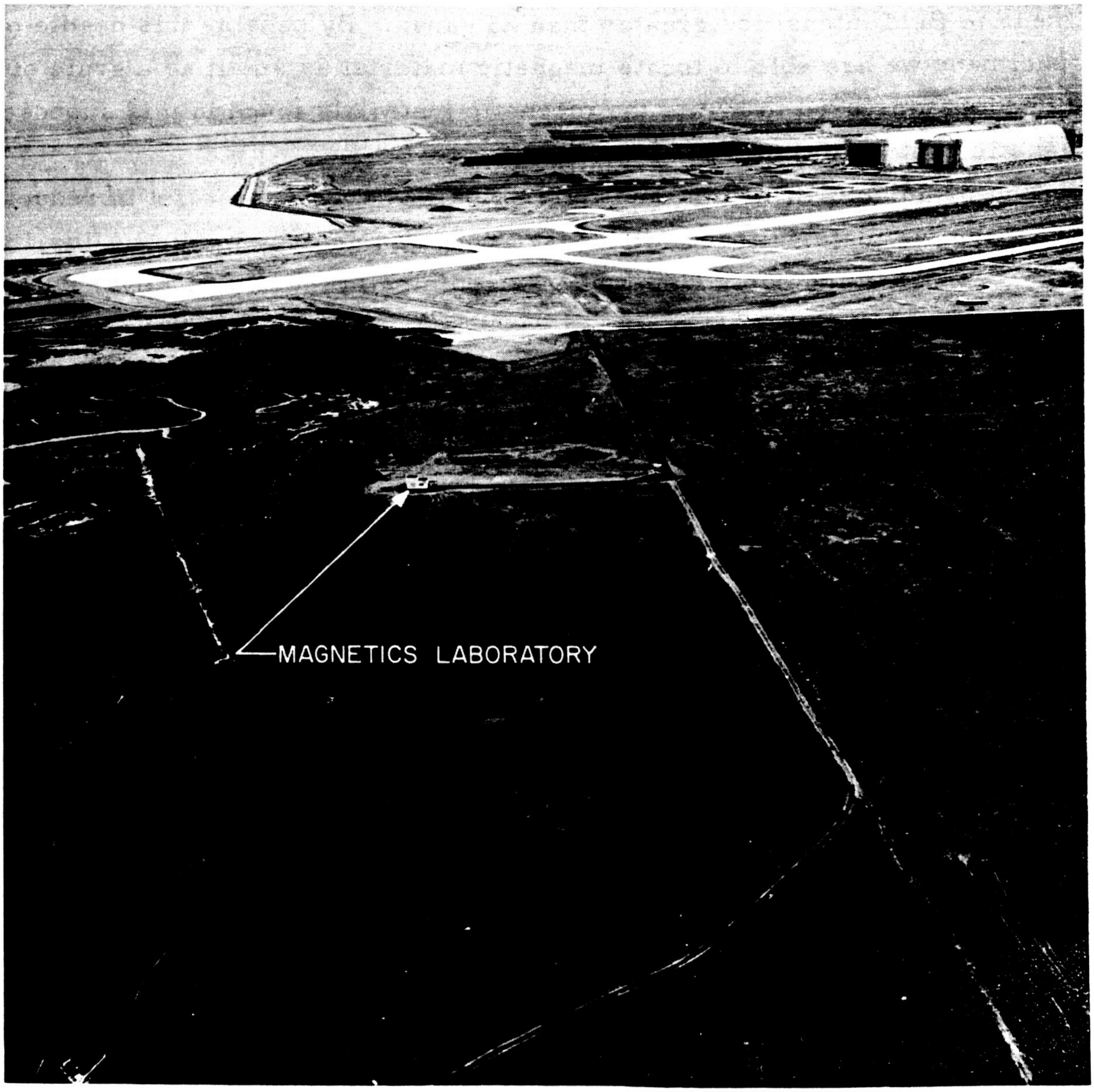


Fig. 4. Ames Research Center Magnetics Laboratory

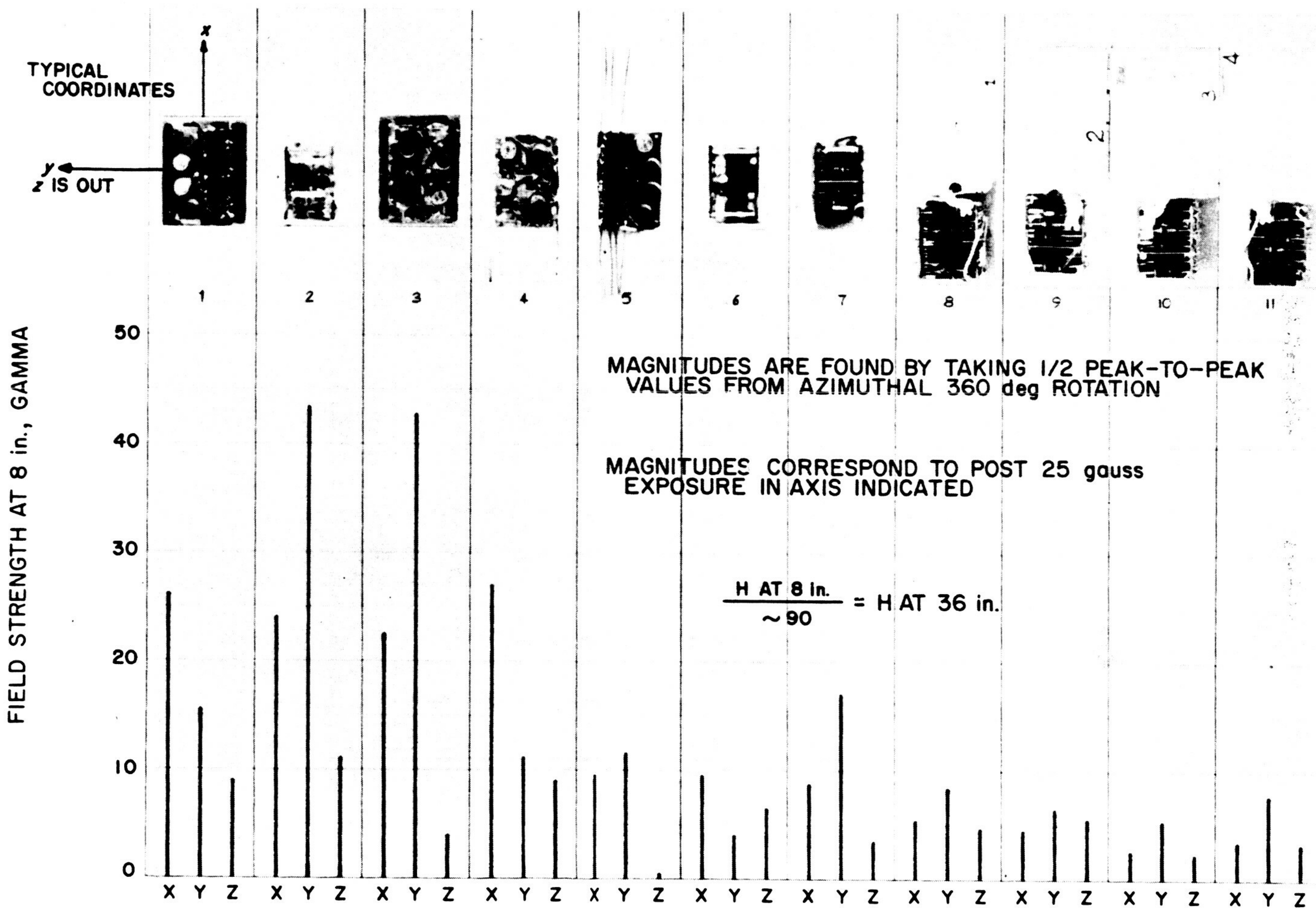


Fig. 5. Magnetic properties of various University of Chicago modules

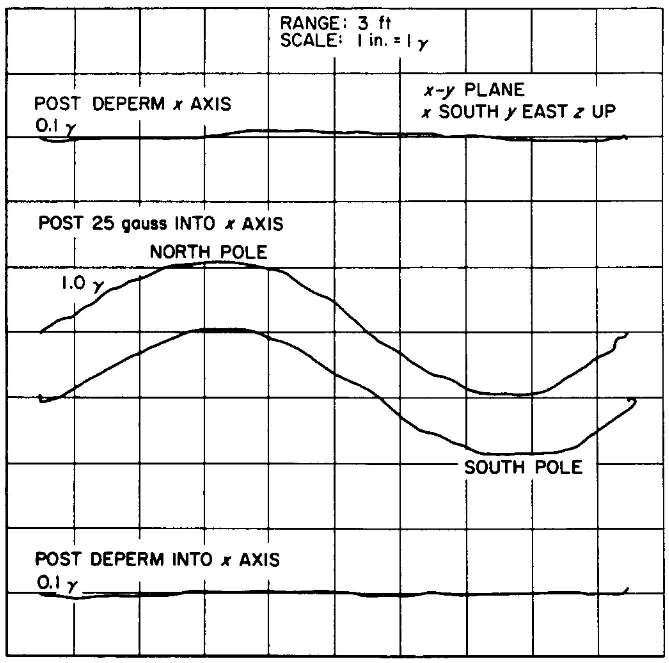


Fig. 6. X-Y plot of magnetic field mapping

In this case we find that after a deperming treatment along the x axis of the experiment, we note a field that has varied during the course of this particular scan from zero to plus 0.1 gamma. After a 25 gauss exposure, the residual field is 1.0 gamma at 3 ft.

You can see the general sinusoid produced by rotating a dipole in azimuth. We see that the field has a closure from left to right of approximately one small division. This is an error in closure of 0.1 gamma. The passband of the instrument system and recorder is 3 cycles in this case.

We see that the peak-to-peak is something like 1 gamma. This corresponds to a peak field of approximately 0.5 gamma measured at 3 ft. Then we deperm and we check the quality of deperm and find that it is essentially a straight line. In fact, there is no correspondence to the sine wave above, so we have depermed it down to something less than 0.1 gamma. The little blip on the trace is probably background noise. This happens to be a recording of the Stanford radio experiment.

We have a magnetic guideline for fabrication. It has been incorporated into the specification for the Pioneer scientific instruments. The title of it is "Magnetic Guidelines for Project Pioneer Experiments," and this could be made available to you if you ask for it. It has to do with definitions of terms and outlines a simple setup for receiving inspection that a contractor can put together with a little investment. It gives guidelines on parts and material selection, how to wire and cable for low magnetic moment, and how to compensate by looping a wire carrying the same current back to make a countermoment.

QUESTIONS AND COMMENTS

MR. BROOK: You mentioned the replacement by these tantalum capacitors of the 350D-type, which most of us are familiar with. What was the offender? Was it the tantalum wire or was it the leads attached to the tantalum?

MR. IUFER: The leads of the 350Ds. At that time we were unable to get a substitute lead from that vendor.

MR. BROOK: I see. One of the reasons why some of us are considering tantalum capacitors is for RF interference suppression, particularly the feedthrough types. These are probably the smallest type of suppressor that we can use, and they

seem to be admirable for satellites. You have sort of thrown that out, and I am just wondering, does this manufacturer make a feeder tantalum? Do you know anything about that?

MR. IUFER: No, we haven't had that problem. In selecting parts, how far you go in developing a nonmagnetic part is determined by how many of those parts you need. If you needed one special lock washer in a 140-lb spacecraft, you might just use a steel one because it wouldn't matter. But, if you were to outfit an entire spacecraft with steel washers, it would probably fail to pass the specification. Its a matter of degree. We needed over 2000 of these capacitors for Pioneer.

So it was well worth our while to go through the exercise of getting parts with a lower magnetic moment.

MR. BROOK: We may need several hundred, I don't know. It might be worth our while.

MR. FRANDSEN: I had a question about the deperming operation. In using the 2-gauss fixed steps, did you run into problems down near zero? I mean which polarity the last cycle was – did you find any residual perm because of this 2 gauss?

MR. IUFER: No, when we reached fields of something like 0.2 gamma peak-to-peak at 3 ft, we were satisfied for the specimen.

MR. PARSONS: I want to take a run at that skeleton deperm treatment. I believe I do understand that these are all equal decrement shots, and the last shot, therefore, is 2 gauss?

MR. IUFER: That's right. Four times Earth's field.

MR. PARSONS: And this is a linear decrement, a linear rate of decay?

MR. IUFER: That's right.

MR. PARSONS: I am perfectly willing to admit (possibly), at 3 ft away for many of the samples that we work with, that 0.2 gamma might result therefrom; but I'm afraid that if I were using DC deperm (I do not any longer; we use 60-cycle almost exclusively on our packages) I am sure that I would feel that I would have to go to exponential decay approaching that zero, and I would take data a little closer than 3 ft.

This brings me to the other question: whether or not it is fair to take data at three times the dimension away from the object. I would say, first, that in 95% of the data we have accumulated, the falloff rate from three to six times has always followed inverse cube for typical packages, if there is such a thing as a typical package.

The one example you mentioned, if I understood the dimensions correctly, was 8 in. measured at 18 in., which is just a little on the low side – two and a quarter rather than three times. I do admit, however, that when the final worst case arises, that is small perm located right at the extreme skin of the package being tested, that the inaccuracies observed at three times and then extrapolated can run up to 10, 12, 14% of that you mentioned.

Well, I will go a little further on the sniffing problems. We found the Hall probe to be useful in sniffing without detriment to the package. It is a little coarse in sensitivity, but when you put that blade right down on it – if it's there you will see it. Another instrument that I haven't heard mentioned much is the gradiometer, which is very useful for parts checking in extremely bad background areas. You can set up a 4-in. gradiometer on a work table in the midst of a factory and pass samples in front of it and very quickly survey those.

VOICE: I heard quite a bit about your capacitors, but I haven't heard you mention relays, and I haven't heard you say a thing about transistor cases. Have you tried to clean these areas up, or do you just live with them?

MR. IUFER: My comments were specifically for the experimenters, which I have personally spent the most time with. The spacecraft contractor has been fighting the relay problem. I think in the command distribution unit they started out with large numbers of latching relays, and now they have reduced the quantity very significantly. They also worked on improving the leakage from the air gaps.

I do not have specific information on this handy, but it is certainly available to anyone who would be interested in our experience.

On deperming, in all my general comments, we test our tests until we find out what we can get by with, and the rules that we have found to be successful may not be refined enough for some people. The length-to-diameter ration on the materials will determine somewhat what you can do in the deperming operation. Also, the ratio between perm and deperm that you require is certainly going to influence whether you are trying to really completely remove all the remanence or not.

Also, in this matter of the level of deperm and decrement, if you have created a perm so that the material is saturated and if you take too coarse a decrement in your deperm operation, you will find that the stability in the depermed state is not good and it tends to revert back to the state at which it was originally magnetized.

So, just that the deperming operation appeared to be a success at the moment is not necessarily indicative that the thing will stay depermed over a long period of time.

They have done quite a bit of work on ships where they depermed and then sampled them over months, and years, and on the basis of this there is some knowledge on stability versus perm treatment.

MR. PARSONS: Agreed, agreed. The old partial shot deperm treatments were seldom very stable.

One other thing. On a tumble exposure and a tumble deperm, I certainly agree that a tumble exposure does exactly as you described, and I, for one, don't care for it. However, the thing that makes it unsuitable for exposure makes it attractive for deperm. Where one must do a deperm treatment in an Earth's ambient background, with no other way of overcoming it, the tumbling action during the 60-cycle (especially deperm treatment), can give remarkably good results.

MR. BENEDICT: Benedict, Jet Propulsion Laboratory. The matter of glass-to-metal seals has been mentioned several times this morning. I understand that your approach is to use a little Kovar eyelet to effectively reduce the quantity of Kovar. I know that comparatively heavy-duty seals, glass-to-metal seals, have been and are being successfully made with the 300 stainless series, which are not completely clean but are incomparably cleaner than Kovar. I am curious, then, as to whether you or one of the other people here might be able to tell me what goes wrong when you attempt to use stainless for headers in comparatively small seals.

MR. IUFER: Well, I can perhaps answer that by asking a question. If you now are able to use alloys that do not necessarily match the thermal coefficient of expansion of the ceramic or glass material, why select a magnetic alloy? Perhaps Nitinol or Alloy 180 could be used. I think too little has been done with the compression sealing techniques, where you do not rely on the thermal coefficient of the metal for the integrity of the seal. We are certainly interested in this area.

MR. GOLDSTEIN: One comment on seals. About two years ago, we had submitted to us for a magnetic test a K-Monel glass-to-metal seal, and it was non-magnetic. Beyond that I don't have any information on it. In looking at a couple of your early slides, it came to mind that it might be worth noting that if a designer is planning to use soldered rather than welded circuitry, there are a whole variety of components open to him that he cannot use if he is going to use welded technology.

For example, capacitors with tin-copper leads, power transistors with beryllium-copper leads are adaptable to soldering. Diodes of several varieties with silver leads are available, and these solder fairly well.

MR. PEIZER: I am not sure whether I heard you right on the allowable ambient during the deperm. I thought that I heard 1% of the Earth's field.

MR. IUFER: Reduced Earth's field by two orders of magnitude.

MR. PEIZER: Is it necessary to go that low?

MR. IUFER: We think so.

MR. PEIZER: We have been talking about tumbling and things, and this is apparently the result of the difficulty.

MR. IUFER: Tumbling is a way of normalizing the uncompensated Earth's field out of the deperm. There is a decided advantage to tumbling, because you not only are concerned about the DC ambient, but we find that we have to deperm about — at least three axes to reduce all the magnetization. A single-axis exposure just won't do the job. By tumbling you essentially are deperming along many axes.

MR. LYNCH: Concerning your previous slide on the wire effect when you cut down the length of wire and showed the reduction of the magnetic moment, I didn't quite get the units of magnetic moment you were using.

MR. IUFER: They were normalized.

MR. LYNCH: What is the magnetic moment of a 1-in. piece of lead wire?

MR. IUFER: We have this on a graph. I am not sure that we brought it with us this morning. We have the information. We actually took magnetic field

me urements at discrete spacing, and then normalized the moments so that you cou see the log characteristic by clipping down to 1/8 in.

MR. LYNCH: Well, I have tried to get some idea of the order of magnitude of the contribution of 1 in. of lead wire, as compared with other effects. If you had this information, it would be helpful.

MR. IUFER: Well, let's see. I would estimate that based on the 3-gamma at 3-in. specification, and the two order or magnitude increase, you might get 300 gamma at 3 in. However, this isn't too good a rule when you have a part with leads 3 in. long.

MR. LYNCH: That would be about 20 pole centimeters?

MR. IUFER: Well, if someone wanted to figure out what H equals $2m/R^3$ you could find the answer.

MR. NORRIS: We had occasion on the Mariner Mars magnetometer to use a tantalum capacitor in the magnetometer sensor itself, and the preamplifier. We ended up having to use a GE29F series capacitor, which has tantalum lead wires coming out of the capacitor body. You cannot solder the tantalum. So we ended up developing a weld schedule to weld palladium to tantalum. This was quite successful in this one application.

Since then, the JPL welding group has been working on palladium weld using palladium interconnects for welding. Of course, palladium is nonmagnetic. It turns out to be compatible with some of the materials that nickel isn't; so you can fairly successfully have nonmagnetic welded modules, as nonmagnetic as your soldered modules.

CHAIRMAN GAUGLER: I would like to make one comment. On Pioneer we have some real prima donnas in experimenters, and we have been surprised how cooperative they have been in reducing their magnetic moment. In fact, paradoxically enough, the magnetometer experimenter is way over specification.

Well, we have a little time for a spontaneous talk and I believe that Mr. Grumet of Republic Aviation wants to tell us about a microgaussmeter.

A MICROGAUSSMETER

Alex Grumet
Republic Aviation Corp.
Farmingdale, Long Island, New York

I would like to describe a microgaussmeter, which we developed on a program for the Navy Bureau of Weapons, that has some unique properties and could find application for making some of the very low-level magnetic measurements in the presence of the Earth's field with a fairly dirty magnetic environment.

We have used the device in that way successfully, and I would like to propose it here to vendors who furnish some of the subassemblies that go into the space packages so that they could inexpensively use this test arrangement, and possibly give their equipment a fairly thorough magnetic testing before it is submitted.

Basically, the device is a rotating coil magnetometer, but the unique feature is that it has no wiping contacts. The coil is a simple single-turn, short-circuited turn that has its plane oriented 45 deg to the rotation axis. The device has extreme sensitivity because the noise limitation in a 6-cycle bandwidth is of the order of 10^{-8} v. The calibration of the unit is about 1 v/gauss so, theoretically, the noise limitation on the unit is 10^{-8} gauss, or about 1 milligamma. I have taken it down to about 10^{-7} gauss at 3:00 or 4:00 in the morning for many minutes, at the time when the Earth's field was not fluctuating too violently, and when the plant was shut down, secured, and no one was moving around within 1/2 mi of the plant.

Now, basically, the operation of the device takes advantage of an insensitive axis. The change of flux linkage is zero if the axis, about which the coil rotates, is aligned with the field being measured. This is true even if the field has a gradient, because what the device does read is the average field that links the single turn short-circuited coil.

We can point the rotation axis along the Earth's field within 1 arc-second, and we know we are within 1 arc-second because the voltage it reads is about 10^{-6} v or 1 μv. If you start with the Earth's field of about 1/2 gauss (rotation axis perpendicular to the Earth's field) and you turn the rotation axis so that the reading drops down to a microgauss, because of the cosinusoidal relationship between field and angle, you can see that you are within 1 arc-second of alignment with the Earth's field.

We corroborated this by mounting the unit on a transit trunnion, a two-axis orienting device, and we can, therefore, point the rotation axis accurately along the

Earth's field. Now, once you do that and you do get the device to read 1 μv, the unit will read any field down to a microgauss, in a plane transverse to the rotation axis. So, if you now move any package that you want to test in the transverse plane, and then if you orient the package until you get a maximum reading, the effective residual dipole moment may be determined; if you consider the package having simply a dipole moment, you will get a maximum reading when you read the radial component of the dipole, which is twice the theta component.

We proceed as above until we get a maximum reading and then we check if we do have a magnetic dipole or a multipole by moving the package under test radially away from the microgaussmeter while the package orientation relative to the microgaussmeter is held fixed.

We have made measurements like this, and because you can read a microgauss, we can make measurements at fairly sizable distances: 10 ft and that order of magnitude for samples with a cubic inch of volume. We have also developed techniques for separating the induced and the permanent or residual field in a sample, which I will describe.

Now, the advantages of this device and the measuring technique described are that you can use the device in an ordinary room. About the only requirement that you do need is that man-made fluctuations in the Earth's field are not too great, which means you either have to work at night when most of the plant is shut down or work in an isolated area.

Where we wanted to make extremely sensitive measurements, we do come in about 3:00 or 4:00 in the morning and work until about 7:00. The minute that the first car comes into the parking lot, we are out of business.

The device is extremely sensitive. We have developed a technique where, if you make a series of these measurements, you can separate the induced and the residual magnetic dipole. To make the separation, the unwanted component is oriented along the insensitive rotation axis of the microgaussmeter and at the same time the desired component is oriented in the sensitive transverse plane. There are, fortunately, enough variables so this can be realized and both the residual and induced fields of a package under test may be separately measured in the presence of the Earth's field. A number of measurements must be made during which the package under test must be located in the transverse plane and in another critically located plane. The orientation of the test package in both planes is also critical. What I will describe will be exactly true for a dipole and will have to be modified accordingly for higher order multipoles.

The magnetic field intensity any distance (R) from a magnetic dipole moment (M) pointed along the z axis and located at the center of a spherical coordinate frame in MKS units is given by:

$$H_\theta = \frac{M \sin \theta}{4\pi R^3}$$

$$H_R = \frac{M \cos \theta}{2\pi R^3}$$

To measure the residual dipole moment only, of a package under test, we align the microgaussmeter for a null and the direction of the Earth's field is then along the rotation axis. The package under test is placed in the transverse plane a distance from the microgaussmeter approximately 10 times the largest dimension of the package to be tested. Before the test is undertaken the direction of the Earth's field should be determined at the site of the test package. If the Earth's field is exactly parallel at both locations, the microgaussmeter will not indicate the induced magnetic dipole moment. This follows because, for a regular shaped test package, the induced dipole moment is in the direction of the Earth's field and θ = 90 deg. Therefore, for the induced dipole moment $H_R = 0$ and H_θ will be a maximum but is oriented along the insensitive rotation axis. It now remains to orient the residual dipole moment in the sensitive transverse plane. This is done by rotating the test sample (at its fixed location in the transverse plane) about two axes until a maximum is indicated by the microgaussmeter. For a fairly regular object, the direction of the induced dipole will remain unaffected (in the direction of the Earth's field) during the rotation but the residual dipole will have its orientation tied to that of the package under test. The maximum reading indicates the direction of the residual dipole to be in the transverse plane and pointed at the microgaussmeter. The direction of the residual dipole is noted on the package under test to be used later.

To measure the induced dipole moment (M_i) alone, a plane at some angle (θ_i) must be located for the location of the test package so the resultant field of M_i lies in the sensitive transverse plane. From Fig. 1 with M_i in the direction of the Earth's

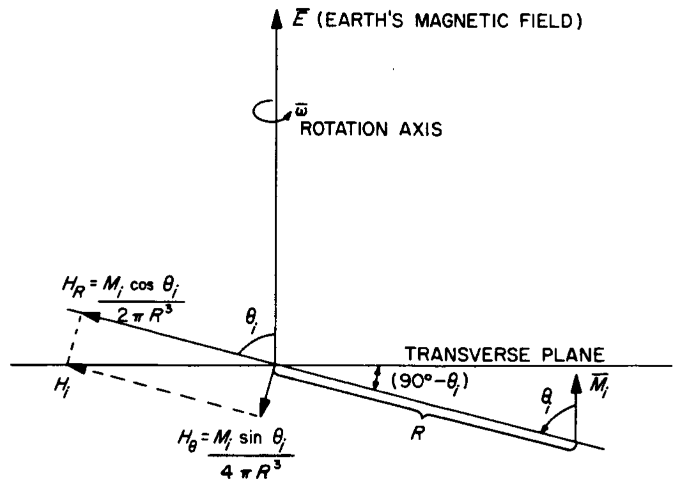


Fig. 2. Vector diagram showing determination of residual dipole moment

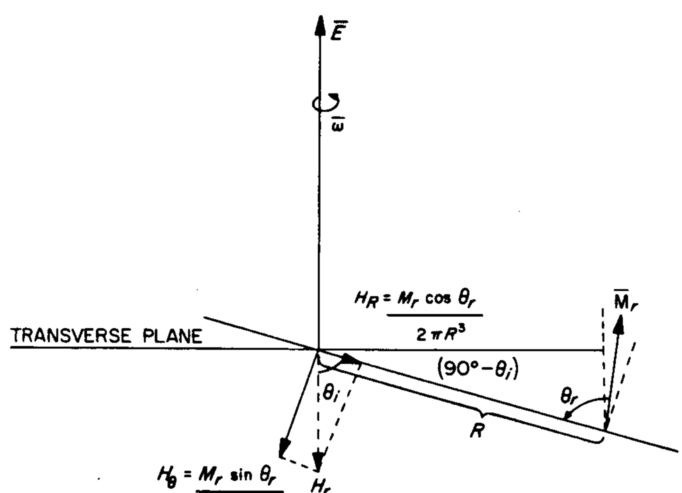


Fig. 1. Vector diagram showing determination of induced dipole moment

field and if the resultant H_i is in the transverse plane, the value of H_i along the rotation axis is zero or:

$$\frac{M_i \cos^2 \theta_i}{2\pi R^3} - \frac{M_i \sin^2 \theta_i}{4\pi R^3} = 0$$

and

$$\theta_i = 54 \text{ deg } 46 \text{ min}$$

$$90 \text{ deg} - \theta_i = 35 \text{ deg } 14 \text{ min}$$

$$M_i = 0.113 \frac{M_i}{R^3}$$

Therefore, reading H_i yields M_i if the residual dipole does not contribute. It now remains to position the resultant field contributed by the residual dipole M_r along the insensitive axis of the microgaussmeter. This is done by proper orientation of the package under a test in the plane at (90 deg - θ_i) to the transverse plane. From Fig. 2 the correct value of θ_r will position H_r along the insensitive axis when

$$\frac{M_r \cos \theta_r \sin \theta_i}{2\pi R^3} = \frac{M_r \sin \theta_r \cos \theta_i}{4\pi R^3}$$

and

$$\theta_r = 70 \text{ deg } 36 \text{ min}$$

Therefore, if the axis of the residual dipole (determined earlier) is oriented in the same plane as the Earth's field and inclinded 70 deg 36 min - 54 deg 46 min = 15 deg 50 min away from the Earth's field, the resultant H_r at the microgaussmeter contributed by M_r, the residual dipole will be along the insensitive axis and will not be indicated.

Unfortunately, most assemblies to be tested are large, so they do not lie completely in a plane. The net result of this is magnetic fields that do not lie exactly

in the transverse plane or exactly along the insensitive axis. The unwanted field component along the insensitive axis is the critical one here and then only to the amplitude of the desired component in the transverse plane. Therefore, for large assemblies, the distance from the microgaussmeter should be made greater than 10 times the largest dimension to minimize this effect. The sensitivity of the microgaussmeter enables the greater distance to be accommodated.

The microgaussmeter is rotated at 210 cps. This speed is selected to fall between harmonics of 60 cps. The narrow bandwidth of the amplifier rejects these harmonics of 60 cps. The output of the microgaussmeter is a 210 cps signal and is read to 1% on a meter. The accuracy of the meter determines the accuracy of the reading. If the meter is a 1% meter, our accuracy is 1% of any scale we use. Therefore, for 1 microgauss full scale we have an accuracy of 1% of 1 microgauss. Speed fluctuations are held to better than 0.5% by a speed regulating loop, locked to a vibrating-reed relay.

Now, another advantage is that if you have to consider the Earth orbiting satellite, which would be moving in the Earth's field, it might be desirable to make measurements in the presence of the Earth's field before launch. Because you want to see what the induced effects might be (particularly for material that has no residual but does have a very high permeability), you might not want to use large bucking coils to null out the Earth's field.

Removing the Earth's field removes the induced permeability distortion of the Earth's field but the technique I have described enables measurements in the presence of the Earth's field.

OPEN DISCUSSION

MR. WOOLLEY: This raises several questions in my mind. Only two of them are of basic importance. First, I am curious to know where you find Earth's field stability over a matter of minutes on the order of 1 milligamma? Secondly, I am very interested to know how you make these measurements of 1 μv levels to 1% accuracy?

MR. GRUMET: Well, the milligamma measurements are only made in a shield, and we use five concentric layers of Mumetal in the shield. We can't possibly do that in the Earth's field, it fluctuates much too rapidly.

To measure 1 μv, at 1% accuracy, there are any number of instruments that will do it. Essentially, what you have is a calibrated narrow band voltmeter — of

which there are several on the market. What we used at first was a Hewlett-Packard, I think it is a 340A wave analyzer. This is, essentially, a superheterodyne receiver with a double crystal filter IF at 100 kc. It has a calibrated output and the entire device is extremely stable. It has a 6-cycle bandwidth with extremely sharp skirts. We have used this quite successfully, but we have recently built our own little package of electronics, much smaller so we can carry it around and have added mobility.

The device, by the way, is rotated by air. We use a little compressor located about 50 or 100 ft away. The microgaussmeter bearings are phosphor-bronze. The copper that we use for the coil was supplied by Anaconda. We asked them to supply copper with nickel, cobalt, and iron less than 0.001%. We ran a spectroscopic analysis on some samples supplied, and, indeed, they were that low.

The machinist had some difficulty machining the copper because it was so soft. He drilled a hole first, forced it on a mandrel, and turned the coil down when it was mounted on the mandrel.

We have used the microgaussmeter as a null sensor in a servoloop and we have bucked the Earth's field (on a single axis) down to about 10^{-7} gauss without any trouble.

We have used speed regulation to 1 cycle by having a little vibrating resonant relay, which we purchased commercially, at 210 cycles; we used this as a frequency or speed reference. One cycle is perfectly adequate because this gives you only 1/2% variation in 210 cycles. It easily allows you to fall in the 6-cycle bandwidth of the filter.

The device gives you an error signal out, which is your 210-cycle frequency. To get sense information for servo application, we put an optical phase reference on the end of the shaft by bouncing light off a half-silvered mirror mounted on the shaft. The housing of the optical phase reference may be turned to provide a phasing adjustment.

MR. GOLDSTEIN: In the study I referred to a couple of days ago, Bob Christy also raised the point, if JPL goes out and tries to buy nonmagnetic components, what do the vendors use to test to make sure they are actually sending us something nonmagnetic. We came up with a real simple, cheap, and dirty type; I have a slide on it I could show you if you like, it is rather interesting. It works on a similar principle.

MR. GRUMET: We have used this device for exactly that application. We take a sample and put it up against the device and we can detect – for example, stainless steels peg the meter; most stainless steels you can't get within 1 ft of this thing. Many cables that have any impurity at all in the center of the conductor have made this device very inoperative. We have used it for exactly that application.

MR. GOLDSTEIN: We are operating in a somewhat lower frequency. I hate to give anybody a free plug, but we are using the Hewlett-Packard 428B with the magnetometer probe. We are spinning at about 5 cps (and using an active bandpass filter from 2 to 8 cycles) to cut out the Earth's field on the low end, 60 cycle and 400 cycle on the high end, and recording on a Sanborn 150 strip chart recorder.

The rotator is a fan motor at the end of a 6-ft phenolic rod. You can use an event marker to trip an external marker for the recorder. The event marker can be something like a nail on the rod driving a microswitch.

We used a compensating magnet to keep the HP 428 on scale. It turns out that the 428B is meter-limited rather than electronics-limited. If you can keep it on scale and just record the AC, you can actually do quite a bit better in resolution than the meter would seem to indicate. We have made measurements on the order of 0.1 gamma on small component parts.

About the only thing that really seems to disturb this is somebody carrying a lambda power supply about 10 ft away. This tears it up, but we have used this in the lab without flux tanks, without coil systems, and it seems to show some promise for an admittedly rather crude method of allowing a vendor to make a quick check.

CHAIRMAN GAUGLER: I want to thank the speakers for doing a bang-up job, and I want to thank our critics for doing a good job, and the audience for being so kind.

ALPHABETICAL LIST OF ATTENDEES AND AREAS OF INTEREST

	Areas of Interest (Refer to list at end)
Donald F. Adamski, Space Physics Division, Aerospace Corporation, El Segundo, California	
H. E. Adelson, Special Projects, Dept 506-10, General Dynamics/Convair, San Diego, California	3, 8, 10
Frank D. Altieri, Hittman Associates, Baltimore, Maryland	5, 7, 9
Harry Angerman, Plant Engineering Division, Jet Propulsion Laboratory, Pasadena, California	1
E. E. Angle, Hughes Aircraft Co., El Segundo, California	
Leonard Arnowitz, Space Exploration Group, Martin Marietta Corp., Baltimore, Maryland	2, 5, 6
B. R. Baker, Manned Space Center, Houston, Texas	
Donald H. Baker, Instrumentation Systems Dept., Sperry Phoenix Co., Phoenix, Arizona	2, 5, 8
Joseph M. Barletta, Electronic Data Systems and Power Dept., Martin Marietta Corp., Baltimore, Maryland	7
James D. Barry, University of California at Los Angeles, Los Angeles, Calif.	
J. G. Bastow, Project Engineering Division, Jet Propulsion Laboratory, Pasadena, California	1
D. E. Beischer, U.S.N. School of Aviation Medicine, Pensacola, Florida	7, 12
A. G. Benedict, Propulsion Division, Jet Propulsion Laboratory, Pasadena, California	

	Areas of interest (Refer to list at end)
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H. Bernstein, TRW-Space Technology Laboratories, Inc., Redondo Beach, California	
R. A. Booth, Guidance and Control Division, Jet Propulsion Laboratory, Pasadena, California	
Kenneth Brantley, Quality Engineering Department, Martin Marietta Corp., Baltimore, Maryland	3, 6
R. S. Brewster, General Motors Defense Research Lab., Santa Barbara, California	
Don L. Broderick, Space-General Corporation, El Monte, California	
Robert H. Brook, AOSO Project, Paul Moore Research Center, Republic Aviation Corp., Farmingdale, Long Island, New York	2, 3, 8
R. E. Brown, U. S. Naval Ordnance Laboratory, Silver Spring, Maryland	
William G. Brown, Test and Evaluation Division, Goddard Space Flight Center, Greenbelt, Maryland	2, 3
William J. Buehler, U. S. Naval Ordnance Laboratory, Silver Spring, Maryland	
E. P. Burt, Northrop Space Laboratories, Hawthorne, California	
Bernard R. Cantor, Fabrication Division, Goddard Space Flight Center, Greenbelt, Maryland	5
A. F. Capridglio, Northrop Space Laboratories, Hawthorne, California	

	Areas of interest (Refer to list at end)
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Bertrand E. Chatel, Space and Information Systems Division, Raytheon Co., Sudbury, Massachusetts	3, 5, 7
J. R. Christy, Project Engineering Division, Jet Propulsion Laboratory, Pasadena, California	
Reid H. Clausen, Electronics Department, Denver Division, Martin Company, Denver, Colorado	2, 3
F. W. Cleary, Hughes Research Laboratory, Hughes Aircraft Co., Malibu, California	3, 5
Paul Cloutier, Rice University, Houston, Texas	3
W. A. Collier, Mariner Mars Project Office, Jet Propulsion Laboratory, Pasadena, California	
B. V. Connor, Space Sciences Division, Jet Propulsion Laboratory, Pasadena, California	3, 7
B. J. Dagostino, Eclipse-Pioneer Division, Bendix Corporation, Los Angeles, California	4, 5, 6, 7
Thomas G. Dalby, Aero-space Division, Boeing Company, Seattle, Washington	
Philip T. Dammann, Electric Products Division, Vickers Inc., St. Louis, Missouri	2
B. C. Daniels, Spacecraft Department, General Electric Co., Philadelphia, Pennsylvania	1, 3
Ernest J. Davenport, Space Systems Division, Los Angeles Air Force Station, Los Angeles, California	
Leverett Davis, Jr., California Institute of Technology, Pasadena, California	

	Areas of interest (Refer to list at end)
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R. J. Debs, Ames Research Center, Moffett Field, California	
Kenneth O. Downing, Department 558-5, General Dynamics/Convair, San Diego, California	3, 5, 8
Paul W. Droll, Systems Engineering Division, Ames Research Center, Moffett Field, California	3, 4, 9
D. P. Easter, National Aeronautics and Space Administration, Washington, D.C.	
C. A. Eberhard, TRW-Space Technology Laboratories, Inc., Redondo Beach, California	
Forrest England, Aerospace Division, Westinghouse Electric Corp., Baltimore, Maryland	5, 9
D. P. Erdmann, Ordnance Division, Honeywell, Hopkins, Minnesota	3, 7
Arthur F. Flacco, Astro Electronics Division, Radio Corporation of America, Princeton, New Jersey	5, 8
Charles L. Fletcher, General Dynamics/Fort Worth, Fort Worth, Texas	4, 5, 9
Walter R. Foley, AVCO R & D, Wilmington, Massachusetts	2, 5, 9
James A. Ford, Magnetic Field Facilities, U. S. Naval Ordnance Laboratory, Silver Spring, Maryland	2, 3, 7, 8
R. E. Fortier, Research Products, Space Division, Chrysler Corporation, New Orleans, Louisiana	3, 5, 6
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	Areas of interest (Refer to list at end)
Howard Friedman, Space Sciences Division, Jet Propulsion Laboratory, Pasadena, California	
Richard M. Friedman, Space Physics Laboratory, Aerospace Corporation, El Segundo, California	7
Harold C. Fue, General Motors Defense Research Lab., Santa Barbara, California	5
A. S. Fulton, TRW-Space Technology Laboratories, Inc., Redondo Beach, California	
Victor Funderburk, Western Development Lab., Philco Corp., Palo Alto, California	3, 9
E. A. Gaugler, Office of Lunar and Planetary Program, National Aeronautics and Space Administration, Washington, D.C.	1
E. H. Gavenman, Lockheed Missiles and Space Co., Sunnyvale, California	2, 5
John J. Ginty, Boston College, Boston, Massachusetts	3
Leonard M. Glasser, Western Development Lab., Philco Corp., Palo Alto, California	3
Dieter Goetze, Systems and Research Division, Honeywell, Inc., Minneapolis, Minnesota	9
Kenneth S. Goldstein, Apparatus Division, Texas Instruments, Inc., Dallas, Texas	
D. I. Gordon, U. S. Naval Ordnance Laboratory, Silver Spring, Maryland	
William S. Goree, Physics Division, Standford Research Institute, Menlo Park, California	3, 4, 7, 12

	Areas of interest (Refer to list at end)
Norman A. Goyette, TRW-Space Technology Laboratories, Inc., Redondo Beach, California	2, 3
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Alex Grumet, Power Conversion & Instrumentation Division, Republic Aviation Corp., Farmingdale, Long Island, New York	2
Jack W. Haas, Goodyear Aerospace, Akron, Ohio	5, 6
B. C. Hall, General Dynamics/Fort Worth, Fort Worth, Texas	
Gordon D. Hall, Special Projects Group, General Dynamics/Convair, San Diego, California	3, 5, 8
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James J. Hayes, Department F510, Avco R & D, Wilmington, Massachusetts	3, 5, 9
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	Areas of interest (Refer to list at end)
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W. M. Hubbard, U. S. Naval Ordnance Laboratory, Silver Spring, Maryland	
Wilson E. Hull, Applied Physics Laboratory, Johns Hopkins University, Silver Spring, Maryland	3, 5, 10
T. C. Hundley, Hughes Aircraft Co., Los Angeles, California	
Ernest J. Iufer, Ames Research Center, Moffett Field, California	1
J. N. James, Lunar and Planetary Projects, Jet Propulsion Laboratory, Pasadena, California	
J. N. Jansen, Antenna and Microwave Group, Motorola, Inc., Scottsdale, Arizona	9
W. E. Johnston, Spacecraft Department, General Electric Co., Philadelphia, Pennsylvania	2, 6
George C. Keller, Goddard Space Flight Center, Greenbelt, Maryland	3, 5, 10
Allan T. Kneale, Motorola, Inc., Scottsdale, Arizona	9
F. K. Lampson, Allegheny-Ludlum Steel Corp., Los Angeles, California	7
Daniel R. Ledbetter, Land Operations Department-Lunar Program General Motors Defense Research Labs., Santa Barbara, California	5
Lynn R. Lewis, Space-General Corporation, El Monte, California	5, 8, 9

	Areas of interest (Refer to list at end)
John W. Lindner, OGO Program Office, TRW-Space Technology Laboratories, Inc., Redondo Beach, California	2, 3, 9
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Joseph B. Lopez, Avco R & D, Lowell, Massachusetts	3, 5
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Francis M. Millican, General Dynamics/Convair, San Diego, California	5
Ronald Moskowitz, Electrical Engineering Department, Rutgers University, New Brunswick, New Jersey	1, 12
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W. R. Murray, TRW-Space Technology Laboratories, Inc., Redondo Beach, California	

	Areas of interest (Refer to list at end)
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A. J. Nalbandian, Project Engineering Division, Jet Propulsion Laboratory, Pasadena, California	
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William O. Nobles, Space Division, Applied Physics Lab., Johns Hopkins University, Silver Spring, Maryland	3, 9, 11
D. D. Norris, Space Sciences Division, Jet Propulsion Laboratory, Pasadena, California	4, 7
Gary L. Ostheller, Project Engineering Division, Jet Propulsion Laboratory, Pasadena, California	3
C. Leland Parsons, Test and Evaluation Division, Goddard Space Flight Center, Greenbelt, Maryland	2, 3
Eugene I. Peizer, Division HK, U. S. Naval Ordnance Laboratory, Silver Spring, Maryland	5, 7
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L. Pode, Space-General Corporation, El Monte, California	
Henry J. E. Reid, Jr., NASA Langley Station, Hampton, Virginia	11
F. N. Reilly, Space Systems Branch, Texas Instruments, Inc., Dallas, Texas	2, 5
Gary I. Roberts, Telecommunications Division, Jet Propulsion Laboratory, Pasadena, California	
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	Areas of interest (Refer to list at end)
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	Areas of interest (Refer to list at end)
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Charles G. Tatosian, Gemini Electronics, McDonnell Aircraft Corp., St. Louis, Missouri	5
Chris Thorpe, TRW-Space Technology Laboratories, Inc., Redondo Beach, California	5
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E. M. Wiler, Quality Assurance and Reliability Office, Jet Propulsion Laboratory, Pasadena, California	
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Areas of interest
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R. P. Woolley,
Ball Brothers Research Corp., Boulder, Colorado

Winfield R. Yeaney, Guidance and Control Division,
Jet Propulsion Laboratory, Pasadena, California

AREAS OF INTEREST

1 Entire area of spacecraft magnetics
2 Magnetic test facilities
3 Magnetic mapping, testing, and measurement
4 Magnetometers
5 Preferred nonmagnetic materials, parts, and processes
6 Magnetic specifications, criteria, and design restraints
7 Magnetic shields and shielding
8 Deperming or demagnetizing
9 Nonmagnetic spacecraft design
10 Flight history of magnetic effects and results of efforts
11 Magnetic attitude control
12 Biomagnetics